

Transatlantic Pressure on Research

BOTH in Britain and the United States, there is a nasty prospect that basic research will be squeezed to an unhealthy degree in the months ahead. The rumours (see *Nature*, **241**, 82; 1973) that the United States Administration is in a mood for yet another halt in the growth of basic science have been strengthened by the report this week (see page 158) that President Nixon intends to carry through the substance of the Ash Commission's proposals for reorganization, if necessary without consulting Congress. The fear, of course, is that the new system will make basic science dependent on the funds which several agencies think they can spare from their day to day commitments. The cancellation by NASA last week of the high-energy astronomy satellite is a sign of how easily the mission agencies can change their minds. Certainly there is no immediate prospect that the National Science Foundation will become, as it should, the chief source of funds for basic science within the federal system. For even if Senator Edward Kennedy is luckier than he has any reason to expect in persuading Congress that his bill, or some amended version of it, should be passed, the President in the present mood will only veto it. And it is only fair to say that the Kennedy bill is as much an attempt to turn the National Science Foundation into an instrument for doing jobs which the federal government at present ignores as for increasing the scale on which the National Science Foundation is enabled to sponsor basic research.

In Britain, the origin of the pressures on basic science is different, but the outlook is also gloomy. Far from seeking to hold public expenditure constant, Mr Edward Heath's government is looking for a greatly increased expenditure of government funds in the next few years but has resolved that most of the extra money shall be spent on social projects such as the creation of jobs for those who are unemployed and the relocation of industry in economically backward parts of the country and on better social services. The result is that expenditure on basic science will take a smaller proportion both of the Gross National Product and of the central government's expenditure than it has done in recent years. Yet the university system is still growing rapidly and the polytechnics are beginning to make demands of their own on the limited funds that are available. The consequences for the universities and for all institutions of higher education will be serious (see *Nature*, **240**, 515; 1972). The prospects for a continuing expansion of fundamental research are more gloomy still. The question to be answered, on both sides of the Atlantic, is how existing institutions should best respond to the impending changes.

One common and urgent need is that the British and the American governments should be made to recognize the potentially important consequences of the policies on which they have embarked. In both countries, there is no obvious instrument by means of which this might be accomplished. In the old days, four or five years ago, there was at least a chance that the National Science

Board, ostensibly the governing body of the National Science Foundation, would emerge as a powerful source of constructive criticism. Events, however, have been a disappointment. It is entirely understandable that a committee whose chief function is to guide in an advisory capacity the officials of the National Science Foundation and, where appropriate, to lobby on the foundation's behalf elsewhere within the government, should be inhibited from having its say in public. For the same reasons, the President's Science Advisory Committee (even if it continues to exist) is bound to argue to itself that discretion is the best defence. Exactly the same considerations have prevented the Council for Scientific Policy in Britain from being an outspoken advocate of the cause of basic science. The Advisory Board for the Research Councils which has now replaced it will be even more discreet, with its membership in which government departments and the research councils will be represented as of right.

Who then is to speak for basic science? In the United States, the National Academy of Science is the obvious choice. By some means that deserves to be understood much better, the academy has managed to live off government funds and to give advice to government agencies without compromising its capacity to speak out in public. Even if the academy's council suffers as much from fuddudduddism as some other comparable organizations, the Committee on Science and Public Policy has a healthy tradition of outspokenness, and the academy's general membership is increasingly alert. In Britain, the Royal Society, for many reasons, some of which are good, is less well equipped to do the job. Is this a role which the British Association (with its promise of support from the Royal Society in the years ahead) might profitably undertake?

What needs to be said? The danger in the present situation is that governments will allow themselves to be carried too far by the swing of the pendulum now under way. Both in Britain and the United States in the late 1950s and early 1960s, governments were persuaded that a growth of basic science would bring industrial innovation and economic prosperity, with the result that science budgets grew quite rapidly. Now their enthusiasm has turned to disappointment, and there is a tendency to take too short a view of the value of basic science. (In this sense, the arguments about the rightness or wrongness of Lord Rothschild's proposals for reorganizing the relationship between the research councils and government departments is irrelevant, for the issue which he raised concerns the management of applied research.) The first thing to be said is that basic research is an essential ingredient of higher education, not merely because it keeps the best teachers on their toes but also because it provides them with something to teach. Who would suggest that the next generation of PhD students should be trained on problems which are either trivial or already understood? The second simple truth which governments must appre-



ciate is that basic research is indeed a powerful source of innovation even if it is sometimes hard to quantify the economic benefits. Even recondite speculations such as the mechanism of pulsating stars (see page 161) are of considerable value to society; if the outcome is a better understanding of the difference between pulsating stars and other kinds of stars, people of all kinds, not merely astrophysicists, will find the Universe a more comfortable place in which to live. But it would be a rash man who would say that a better understanding of how stars function may not in the long run be of economic benefit. The practical problem, therefore, is the present lack of criteria, agreed between governments and the scientific community, about the scale on which basic research should be carried out. Before anything much is said, there is a need for a careful investigation of the place of basic science in modern life and then for a moderate but forceful statement of the need.

Other institutions within the scientific community can do a lot to help. Within the university system, but especially in Britain, it is important that scientists should recognize the need for selectivity and concentration. No great harm would come about if it were openly acknowledged that only some university departments in particular fields of science were important centres for research (which does not of course imply that academics in other departments need or should opt out of scholarship). In Britain, this is especially important now that the polytechnics are bent on following universities along the path of uniformly distributed research funds. The grant-giving committees of the National Science Foundation and the National Institutes of Health in the United States, and of the research councils in Britain, have a similarly important part to play in implementing such a policy. And for as long as the budgetary pressure on research funds remains as powerful as it is, the temptation to secure a lien on government funds by the setting up of research institutes separate from universities should be resisted. Especially because policies like these will require that individual researchers should frequently be prepared to forgo personal advantage for the common good, it is of the utmost importance that the steps to be taken to meet the pressures on research should be publicly discussed and widely agreed.

Dr Medvedev in London

THE Soviet government has done well to allow Dr Zhores Medvedev to spend the coming year at the National Institute for Medical Research at Mill Hill, London. For several years Dr Medvedev has been campaigning for more liberal arrangements for Soviet scientists to communicate with colleagues elsewhere, and has suffered much inconvenience as a result. Perhaps the Soviet government has wryly calculated that it will be best for both of them if Dr Medvedev now spends a year rusticated in Britain and there is always of course the chance that he may find the traffic is not an unmitigated blessing, that even Western scientists are sometimes over-reticent and that the common concern about research budgets can depress the spirit. Yet the Soviet government should know that giving a passport and an exit visa to Dr Medvedev and his family will go a long way to ease

relations with the scientific community in the West. For Dr Medvedev has been something of a test case, and in the past few months there has been much muttering about the wisdom of participating in international conferences organized in the Soviet Union. This should now be in large measure dissipated (which is not to say that Western scientists would not welcome much less formal means of spending time with individual Russian scientists and of communicating with them). And it goes without saying, of course, that if the Soviet government's liberal action in the past few days should be followed by a revocation of his exit visa, unthinkable though that may be at present, there will be a howl of protest and lasting damage to cultural relations with the Soviet Union will be done.

By his own account, Dr Medvedev is a Russian patriot and is determined to return to the Soviet Union when his year is up. It is therefore important that he should be helped as much as possible to devote himself single-mindedly to his research, and that he should not be drawn into situations which could be interpreted as disloyal to the Soviet Union. For, however much people in the West may think it their right to go abroad and criticize their governments, it is well known that the Soviet government takes a different view.

100 Years Ago



THE NAVY AND SCIENCE

IT would be difficult to estimate the many excellent effects that are likely to result from the establishment of the Royal Naval College, which, as has been at last authoritatively intimated, is to be opened on February 1, in those noble halls at Greenwich that for so long have been associated in another way with the British Navy. Her Majesty's Government deserve the highest praise for the wisdom—provokingly tardy though it has been—displayed in the thorough and handsome provision they have made for the scientific education of our naval officers. Much that is sarcastic, no doubt, might be said on this tardiness of a Government which seldom moves until it is driven; but as we fear this would do little good, we shall only express a hope that in future when they are compelled to take action in any matter, especially if it be scientific, they will do so as decidedly and sweepingly as they have done in the present instance.

Movements of the Earth's Surface

It is, I believe, commonly supposed by geologists that the movements of the surface of the earth are caused by the refrigeration and contraction of the interior. But since the glacial epoch the surface of the earth has become warmer; consequently since that time a heat wave must have been passing from the surface towards the centre; and consequently since that time no refrigeration nor contraction of the interior can have taken place. If, therefore, movements of the earth's surface were due to this cause only, no such movements should have taken place since the glacial epoch.

Wellington, New Zealand, Nov. 10, 1872

F. W. HUTTON

From *Nature*, 7, 217, January 23, 1873.

OLD WORLD

University Interest Aroused

SEVERAL university departments have submitted applications to the Science Research Council for grants to work on superconducting a.c. generators following a report by an SRC working party on the subject last November (see *Nature*, **240**, 175; 1972). Among these are the Universities of Oxford, Southampton and Leeds; other requests for support are on the way.

The Oxford application is from the mechanical engineering department to work on the cryogenic aspect of a.c. generation. Southampton's interest comes from both the mechanical and electrical departments, and the Leeds application is also from the engineering department.

Those applications that reached the SRC's Electrical and Systems Engineering Committee by the last closing date for applications (December 15, 1972) will be considered by the committee in March after the working party, which is to be reconvened, has offered its opinion on them in February, and after the council, in the same month, has decided its attitude to the working party's report and how much money to make available.

Other applications which reach the SRC by its next closing date for applications, the beginning of April, will be considered in that month, and the committee hopes to see at least three or four groups working on the lines recommended in the report by the end of 1973, although a spokesman for the SRC was unwilling to discuss individual applications last week.

When the committee circulated the report, it asked for comments on it and for suggestions of other fields to which superconductivity could be applied. Reaction so far has been largely favourable. Most consider that the report is a reasoned assessment of the situation, although some feel that the case for initiating research on superconducting generators has not been proven. Nobody, it appears, is of the opinion that the working party was too timorous in its suggestion that research should start quickly, but on a comparatively small scale.

The request for suggestions of other areas to which superconductivity could be applied has been disappointing. One or two ideas have been put forward, but with the exception of a.c. generation and d.c. motors, superconductivity at present appears not to be as useful a phenomenon as it seems to be at first glance.

The Electrical and Systems Engineering Committee is also examining other areas of its brief in an effort to redefine the direction of its work. A report on electrical machines other than superconductors is due shortly, and the committee is also to examine work on solid state devices and telecommunications.

But superconductivity looms largest on the committee's agenda at present, as it does at International Research and Development in Newcastle where £1.5 million has been spent on research into a.c. and d.c. superconducting machines in the past ten years.

IRD, under the enthusiastic guidance of Mr A. D. Appleton, its head of electrical engineering, has undertaken much of the industrial superconducting work in Britain. Its greatest success has been with d.c. machines, but since 1968 a.c. generation has also played a large part in its work. The company will be closely involved in the planning and coordination of the research on a.c. generation that the SRC is to

finance, for one of the strongest stipulations of the working party's report is that research must be closely linked to industry.

IRD has examined generator designs up to 2,000 MW, and is already talking to many of the academics interested in undertaking work in the field. IRD—as was made plain by the SRC working party on which Mr Appleton represented the company—is eager to build a 60 MW generator, at a cost of about £2 million. Such a machine would be by far the largest superconducting generator ever built, the only competition being the Westinghouse 5 MW machine. Mr Appleton points out that a 60 MW machine is big enough to give a real idea of the problems involved (unlike a 5 MW machine) as well as having the advantage that it can be installed in a power station to replace one of the 60 MW sets that the Central Electricity Generating Board is currently phasing out, thus providing genuine working conditions and removing the not inconsiderable problem

OPEN UNIVERSITY

First Degrees Awarded

THE Open University announced the award of its first degrees last week. Eight hundred and sixty-seven students have qualified for the university's BA degree and can now go on to acquire an honours degree after a further one or two years.

The Open University is at pains to point out that the 867 are atypical students. All have passed their degree in just two years, acquiring the six credits necessary by being given two or three credit exemptions (for previous academic qualifications) and by obtaining two further credits each year—the maximum. Next year's graduates should be much more typical.

The university has remarkably few statistics available about its graduates. These are all in the computer at present is the claim, so that it is not even known whether the new graduates are predominantly science-based or arts-based.

Seven out of ten of the university's other students gained credits in the 1972 examinations. A total of 36,076 foundation and second level examinations were taken by 25,203 students; 23,578 passed one or more course examinations, thereby gaining a credit, and 33,458 credits were awarded in all. The overall course pass rate for the year

was 71.4 per cent for foundation level courses and 70.5 per cent for second level.

While it is not yet possible to calculate how many students have dropped out of the Open University since it started—students can accumulate the credits needed for their degrees over several years—the dropout rate on courses is less than 25 per cent. Seventy-eight per cent of students who started courses took the examinations, and of those who took them, 91.7 per cent passed.

The Open University is well pleased with the results which are published just as it launches the largest part of this year's advertising campaign to encourage applications. About £20,000 of the £35,000 advertising budget is to be spent in the next few weeks on advertisements in regional and national newspapers and on the university's first venture into television commercials. Advertisements will be run on Border Television starting this week and their effect assessed as a pilot scheme for a fuller use of television. Although advertisements will appear in more than fifty national and local papers, the university is concentrating its efforts on those parts of Britain which have had a poorer application rate so far—the north, Wales, the west midlands and the north-west.

of finding a suitable test load. The snag at the moment is finding £2 million.

On d.c. motors, however, IRD has progressed much further. The biggest remaining problem, according to Mr Appleton, "is persuading people to accept a new technology". IRD's 3,250 brake horse power motor successfully drove a water pump at Fawley power station for four months in 1971, although problems with the refrigeration unit hampered work. IRD's current project involves building a 1 MW propulsion unit for the Ministry of Defence which will undergo trials at IRD shortly and will be put into a naval ship later this year. The British Steel Corporation is also interested in using a superconducting motor to power its steel mills, and if that becomes a reality, and is successful, the day may not be so far away when superconducting d.c. motors become a standard part of industry's equipment.

EDUCATION

University Physics

THE Nuffield Foundation has allocated £55,000 to bridge the gap between A-level and first-year university physics. The grant made on January 1 is for five years and is allocated on the basis of a successful pilot study carried out in 1971 and 72 at the physics departments of the universities of Birmingham, Keele, Surrey, York, Chelsea College at the University of London and University College, Cardiff.

Mr R. A. Sutton of the physics department at Cardiff, the only full-time person working on the project at present, said this week that there was an increasing problem of matching the wide range of backgrounds and ability of sixth formers to the needs of university first-year physics courses. A-level physics syllabuses vary from examination board to examination board and it is common for students to omit some parts of the syllabus when studying for their examinations. Such gaps in knowledge only become important when further work is being built on the missing parts in university. But, according to Mr Sutton, a much more important factor is the differing emphasis laid on the syllabus by school teachers.

With the problem areas now identified, during the next five years the project will be directed towards developing self-teaching units of various kinds to help students over the transition period between school and university. Efforts will also be made to refine the diagnostic procedures and the effectiveness of the self-teaching units will be analysed as the project proceeds.

The self-help material is in no way, said Mr Sutton, to be construed as replacing the A-level syllabus, and it is hoped that one of the side benefits of the project will be that university teachers will devote some time to thinking about how physics is taught. Some of the grant may also be spent in appointing temporary lecturers so that the regular staff can spend their undivided attention in preparing self-teaching manuals, or to appoint people to help the regular physics department staff to prepare the aids.

The project is guided by a committee consisting of Professor C. A. Taylor of Cardiff, Professor E. J. Burge of Chelsea College, Professor S. J. Eggleston of Keele, Professor O. S. Heavens of York and Professor K. W. Keohane of Chelsea College. It is not the first attempt to try and correlate a subject in school and university—the Nuffield Foundation has supported a similar project in biology since 1969.

CHESS

Hartston Misses Narrowly

THERE is a long tradition going back to 1895 of International Chess Tournaments held in the town of Hastings, but in some ways the ending of the most recent tournament which was held during the past three weeks has proved to be one of the most interesting of all.

The tournament was won by Bent Larsen of Denmark, who is certainly one of the most successful tournament players of recent times. Larsen played with the utmost aggression and self-assurance and scored 11½ points from his 15 games. Occasionally his style allows for some drastic defeats, and he was decisively beaten at Hastings this year both by Uhlmann of East Germany and by Radulov of Bulgaria. These defeats, however, did not deter Larsen from winning no fewer than 10 of his remaining games, including wins from each of his last four games in the tournament, a time when lesser players begin to tire and look for comparatively restful draws.

The East German, Uhlmann, ran Larsen a close second with 11 points, and when on form (as he was at Hastings) is clearly a formidable player with a similar fighting spirit to Larsen.

One surprise of the tournament was the performance of the Englishman, William Hartston, who came a clear third. Hartston, at 25, has only just qualified for his International Master title and at Hastings only narrowly missed obtaining the first leg of a "grand master" qualification. The measure of his performance can be gauged by the fact that he finished above six recognized international grand masters (including one ex-world cham-

RESEARCH AND DEVELOPMENT

Ways and Means

PROFESSOR H. B. G. CASIMIR, president of the European Physical Society and until recently head of research at Philips, Eindhoven, publicized his reservations about the customer-contractor principle as applied to scientific research in a lecture given at the Institution of Electrical Engineers last week. He proclaimed that "scientific originality cannot easily come from a sense of duty" and went on to say that in his experience the best scientific work is usually done by a person straying a little from his usual discipline to involve himself in a subject that fascinates him. The inconvenience that this may cause, Professor Casimir said, is the price that must be paid for a first-class mind.

Speaking of the relationships between technology and basic science, Professor Casimir drew attention to the way in which the dependence of one on

pion), and that as late as the thirteenth round had been joint leader of the tournament. His last two games, however, resulted in losses to the eventual first two prizewinners, Larsen and Uhlmann, but his score of 9½ points was still an excellent one.

Perhaps the greatest surprise of the tournament, however, was the relatively poor showing by the Russian representatives, Tukmakov and Smyslov, who finished only in equal fifth and ninth places respectively. At last year's Hastings Tournament, the two Russian players, Korchnoy and Karpov, finished in equal first place, but since the strafing of the top Russian players by Fischer in recent times a slightly unsettling effect has been noticed in the whole Russian camp of chess players, and their play has been rather less convincing than in previous years.

The following game is the critical and hard-fought fourteenth round game between Hartston and Uhlmann.—J.P.

Hartston (white) v Uhlmann (black)

FRENCH DEFENCE

White	Black	White	Black
1 P-K4	P-K3	27 Q-Q4	P-R5
2 Kt-QB3	P-Q4	28 P-R4	PxP
3 P-Q4	B-Kt5	29 QxQ	R(Q1)xQ
4 P-K5	Kt-K2	30 BxQBP	R-Q7
5 P-QR3	BxKt ch	31 R-B2	RxR
6 PxR	P-QB4	32 KxR	K-B2
7 Kt-B3	B-Q2	33 P-R5	Kt-B1
8 P-QR4	Q-R4	34 B-K5	R-K1
9 Q-Q2	QKt-B3	35 B-Q6	Kt-Q2
10 B-K2	QR-B1	36 P-Kt4	PxP
11 PxP	Kt-KtP	37 P-B5	Kt-B1
12 O-O	QKtP	38 K-Kt1	K-B3
13 Kt x Kt	Kt x Kt	39 B-Kt ch	KxP
14 Q-K3	Kt-Kt3	40 BxP	Kt-Q2
15 B-R3	B-B3	41 B-Q3 ch	K-Kt4
16 P-KB4	O-O	42 KxP	KxP
17 Q-R3	QR-Q1	43 R-K2	K-Kt4
18 B-QKt4	Q-B2	44 B-Q4	B-B6
19 B-Q3	P-B4	45 R-Q2	Kt-B1
20 Q-Kt3	R-B3	46 B-Q3	Kt-Kt3
21 Q-K3	P-QR4	47 R-R2	P-K4
22 B-R3	BxP	48 B-K3 ch	Kt-B5
23 B-Kt2	B-B3	49 R-R7	P-R6
24 P-B4	R-B2	50 B-K4	K-Kt3
25 P-KKt3	R-K2	51 R-R2	R-QR1
26 QR-K1	Q-Q2	52 P-B3	B-Q4
		White resigns	

another has changed. Steam engines were well established before thermodynamics as such saw the light of day, but the situation soon reversed and the properties of electrons, for example, had to be understood in some measure before any kind of electronic device could be constructed. At present, industry rarely uses the most recent advances in basic science, however, and the "lead time" is often of the order of fifteen to twenty years. On the other hand, scientists, especially astronomers, who make use, for example, of satellite systems, manage to get hold of the most recent technology. And often the lead time is almost zero.

Most scientists, Professor Casimir said, spend much of their time filling in gaps rather than making fundamental discoveries. But when they are filling gaps for the benefit of industry, they must avoid excessive control by the interested industrial concern, which must also resist the temptation to be overbearing.

Professor Casimir also described the different ways in which a company can organize its research and development. First, there is an arrangement along the lines favoured by Bell Telephone Laboratories in which a central research and development organization is responsible directly to the board of management of the company. One objection to this is that the manufacturing units within the company have too little say in the formulation of research policy. Second, each manufacturing unit within a company can make its own research arrangements. If there are n units then there are $\frac{1}{2}n(n-1)$ interfaces between research departments across which meaningful dialogues have to be conducted if advantage is to be taken of work done by the others. Third, there is an arrangement with n interfaces, namely one in which all units have facilities for development but where there is also a central organization for advanced research and development. This is the scheme adopted by Philips, and it has the edge, as far as problems of communication are concerned, when n is four or more, that is in a moderately diversified company.

ITINERANTS

Medvedev Arrives

ZHORES MEDVEDEV arrived in Britain this week to take up a one-year appointment at the National Institute for Medical Research at Mill Hill. He arrived by train on Sunday accompanied by his wife, who is also a biochemist, and his 16-year-old son. His eldest son is still in the Soviet Union.

Dr Medvedev said this week that he is looking forward to his year in Britain

and he is hoping that some experiments can be carried out at Mill Hill to try and test his theories on the mechanism of the ageing of cells.

The visa that was issued to Dr Medvedev on December 12, 1972, is valid for one year but the family's passports are valid for two years. It is possible in principle, said Dr Medvedev this week, for the visa to be extended for a further year but it is too early yet to say whether his work will make it necessary for him to apply for an extension.

Dr Medvedev does not know whether he will visit other countries during his leave from the Soviet Union. He has had invitations to attend conferences both in Europe and in the United States but before making any arrangements he will find out from the Soviet Embassy whether his visa is valid for travel to countries other than Britain. He particularly would like to visit Berkeley, California, in August to attend the 13th International Congress on Genetics.

ASTRONOMY

Plumian Professorship

THE saga of astronomy in the University of Cambridge had another chapter added this week with the news that Professor Martin Rees is to become Plumian Professor of Astronomy and Experimental Philosophy at the University.

Professor Rees, who only took up his present position as Research Professor of Astronomy at the University of Sussex in October 1972, was previously at the then Institute of Theoretical Astronomy at Cambridge as a research fellow in Professor Sir Fred Hoyle's group.

But in the past year astronomy at Cambridge has suffered much publicity and some losses. In April 1972, Sir Fred announced that he was resigning from the Plumian Chair, and speculation at the time ascribed this to, among other factors, his disagreement with the reorganization of astronomy teaching and research at Cambridge (*Nature*, **236**, 419; 1972).

Under the new arrangements, which came into force last August, the Institute of Theoretical Astronomy and the University Observatories were merged into the Institute of Astronomy, and Professor Rees will be one of the three professors at the institute. Each professor in turn is expected to hold the directorship of the institute; the present director is Professor Donald Lynden-Bell, who himself only took up his position as Professor of Astrophysics at the university last October.

The University of Sussex will undoubtedly not be pleased to lose a professor so soon after his appointment, but nobody will begrudge Professor Rees his appointment to the premier chair of astronomy in Britain at the age of 30.

Alice in Eco-land

"I SEE" said Alice "that the *Sierra Club Bulletin* has an ad for boots made of mustang leather, and a long article asking us to protect wild horses. Why?"

"Because" said the Mad Hatter "the word 'mustang' in the ad only means that the boots are mustang colour. The leather is cowhide, so everything is ecological."

"But" said Alice "cows spend their lives giving us milk. They are very friendly. Why do we make them into boots and hamburger, if we protect mustangs?"

"Cows are not an endangered species," replied the Hatter, "they are only endangered individuals. They are not part of the web of life. So they don't matter."

"Why are mustangs an endangered species?" asked Alice. "They are just horses that have run wild. There are lots of tame horses. If a cow ran wild, would it be an endangered species?"

"No," said the Hatter. "It would be rounded up, and made into hamburger."

"If I were a cow" said Alice "I

would ask a genetic engineer to make me into a mustang."—*T. Jukes, University of California, Berkeley, California.*



NEW WORLD

Science and the Executive Branch

by our Washington Correspondent

EARLY in 1971, President Nixon asked Congress to approve far-reaching plans to reorganize the federal bureaucracy along functional lines as recommended by the Ash Commission. The heart of his proposals was the amalgamation of several departments and agencies into four conglomerate departments to be called the Departments of Natural Resources, Human Resources, Community Development and Economic Affairs. But the plans died in the committee rooms of Capitol Hill and did not get past either the House or the Senate.

President Nixon has served notice, however, that he will again send essentially the same reorganization plan to Congress in the next few months. He has also put into effect some changes in the executive branch which move as close to his reorganization plan as possible without requiring congressional approval. The changes to some extent involve the planning of science and technology, and they also provide some pointers to the possible fate of the Office of Science and Technology—an issue which was still the subject of considerable speculation and uncertainty at the end of last week (see *Nature*, 241, 82; 1973).

On the domestic front, President Nixon has set up three policy-making committees, each of which will be run by a cabinet member and which will in effect be committees of the Domestic Council—a committee set up in the White House in 1969 to advise the President on domestic affairs. The committees will be concerned with natural resources, human resources and community development; they will be composed of the heads of departments and agencies whose activities fall in those categories and their function will be to plan and coordinate policies.

The chairman of each committee will become a counsellor to the President, and it takes no stretch of the imagination to see that their roles will come close to the roles prescribed for the heads of the three conglomerate domestic departments which President Nixon wants to set up.

Mr Earl Butz, Secretary of Agriculture, will take on the additional post of Counsellor for Natural Resources. Mr Caspar Weinberger, the Secretary-designate of Health, Education and Welfare, will also become Counsellor for Human Resources. Mr James Lynn, the Secretary-designate of Housing and Urban Development, will be-

come Counsellor for Community Development.

In addition, John D. Ehrlichman, Chairman of the Domestic Council itself, will provide liaison between the new counsellors and the President, and George Shultz, the Secretary of the Treasury, was last month given the task of planning economic policies in much the same way as the head of the proposed Department of Economic Affairs.

Ehrlichman said last week that the committees will be concerned with policy issues which at present involve several different departments and agencies. In the past, he said, such issues have been dealt with by White House staff because they have been the only people able to bring together the views of officials in several departments. "This is, in effect, forcing down out of the White House to the cabinet members themselves—off the President's desk, onto the counsellors' desks—these policy development issues," he said. One result will be a 50 per cent reduction in the size of the White House staff.

If the counsellors' areas of responsibility match those of the heads of the proposed conglomerate departments—and Ehrlichman confirmed that such would be the case—Butz's committee will be concerned with such issues as energy policy, resource development and conservation. Weinberger's committee will be concerned with most of the present functions of the Department of Health, Education and Welfare, some of the functions of the Department of Agriculture such as the school lunch programme and meat and poultry inspection and some functions of the Department of Labor. And Lynn's committee will be concerned with most of the functions at present being performed by the Department of Housing and Urban Development, together with policies concerning mass transportation and highway construction.

How does science and technology fit into this new structure? The first thing to be said is that the executive reorganization plan submitted to Congress two years ago would have had little effect on most of the agencies with primary responsibility for science and technology—the National Science Foundation would scarcely be affected, for example, nor would NASA. But the plan does involve significant changes in the Atomic Energy Commission, the National Oceanic and Atmospheric

Administration and the scientific activities of those departments which would be grouped together in the super-agencies.

The original idea was to move NOAA lock, stock and barrel into the Department of Natural Resources, and so presumably one of Butz's responsibilities will be to help integrate its policies with, for example, those of the Fish and Wildlife Division of the Department of Interior and the soil and water conservation services and forest services of the Department of Agriculture.

As for the Atomic Energy Commission, its civilian power, raw materials, enrichment and Plowshare programmes were scheduled to be moved into the Department of Natural Resources and so Butz will also have a finger in those pies while the chairman of the AEC, when he is appointed, will presumably get a seat on Butz's committee. One objective of that committee will clearly be to develop an energy policy for the United States.

The reorganization leaves aside the question of what would become of the AEC's physical research and weapons development divisions. Neither the plan submitted to Congress two years ago nor the charges announced last week had anything to say about those activities.

One aspect of Butz's new appointment has raised some muted comment from environmentalist groups. The chief responsibility of his committee will be to develop policies for resource utilization, and many of the issues it will consider involve environmental considerations. What, then, will be his relationship with the Council on Environmental Quality, which is charged with the task of overseeing the development of environmental policies?

President Nixon announced last week that all three members of the CEQ have been reappointed for his second term, thereby laying to rest rumours that he would dismantle the council, but whether it will be able to hold its own with Butz's high-level committee remains to be seen. In any case, the fears of the environmentalists will not be lessened by a statement made by Butz at a press conference last week that "when we run short of power, the first people to have their power shut off should be those who blocked the Alaskan pipeline".

One body which is also likely to be profoundly affected by the reorganization announced last week is the Office

of Science and Technology. For one thing, the committees of the Domestic Council which have been set up are likely to concern themselves with some questions of science and technology which would normally have fallen in the purview of OST—the development of an energy policy, for example. And for another, much of the work of OST during the past two years has been concerned with the relationship between investment in research and development and general economic policies. Such questions would presumably fall into the area of responsibility of Mr Shultz and the proposed Department of Economic Affairs.

These considerations, coupled with the fact that Dr Edward David and Dr John Baldeschwieler, the Director and Deputy Director of OST respectively, both left the White House earlier this month, have increased speculation that OST will be scrapped. One possibility which is now being voiced is that some of the staff of OST will be moved over to the Office of Management and Budget, while others may serve on the staffs of the committees of the Domestic Council. No plans have been announced, however, and members of OST are anxiously awaiting news of their future.

SCIENCE POLICY

Help for Kennedy

by our Washington Correspondent

A BILL was introduced into the House of Representatives last week which could greatly influence the prospects for Senator Kennedy's National Science Policy and Priorities Act. Numbered HR 32, the bill proposes essentially the same programmes and objectives as Kennedy's bill (which is numbered S 32), but attempts to overcome one of the chief objections that has been raised against Kennedy's proposals—that they would undermine the responsibility of the National Science Foundation to foster basic research.

Introduced by Representative Alphonzo Bell, a Republican member of the Subcommittee on Science, Research and Development, the bill would create two new agencies within the National Science Foundation—the Civil Science Systems Administration which Kennedy proposes and a Science, Research and Education Administration. The idea is for the Civil Science Systems Administration to sponsor applied research and development to solve urban problems, while the Science, Research and Education Administration would be concerned with the support of basic research and education. An important part of Bell's proposals is that the latter agency should receive at least 40 per cent of NSF's total budget and that

no funds could be transferred between the two branches of NSF.

Bell says that he has introduced the bill to meet objections raised during hearings on Kennedy's bill conducted by the Subcommittee on Science, Research and Development last year (see *Nature*, 239, 302; 1972). But his bill retains the proposal that spending on civilian science and technology should equal that on defence science and technology, and calls for a total expenditure of more than \$1,000 million over three years. It would therefore probably be vetoed by President Nixon if it were passed.

DRUG LICENSING

FDA at Law

by our Washington Correspondent

THE US Supreme Court has decided to resolve a legal conflict between the Food and Drug Administration and the several drug manufacturers who have challenged the FDA's procedures for taking ineffective drugs off the market. The future of the FDA's drug control programme could well hang on the court's decision.

The Supreme Court will review five cases which have recently been tried by lower courts and which turn on the question of whether the FDA is obliged to hold public hearings when it bans supposedly ineffective drugs. Also at issue is the scope of the provision in the food and drug laws which requires drugs to be effective and the role of the courts in deciding manufacturers' appeals against FDA actions. The Supreme Court is expected to review the cases in the spring

and to hand down a decision by June.

The basis of the court cases is an amendment to the food and drug laws passed by Congress in 1962. Before then, the FDA had power to remove from the market only drugs judged to be unsafe. The 1962 amendment, however, gave the agency the power to require that drugs be effective as well as safe and the FDA set in train a review of the efficacy of the 4,000 or so drugs that it had then approved.

The review, carried out by the National Academy of Sciences, has already led the FDA to withdraw approval for more than 1,300 drugs. Such numbers would clearly be difficult, if not impossible, to handle under the agency's standard regulations for removing products from the market, which provide manufacturers with the right to a public hearing if they wish to contest the FDA's actions. The agency thus published new regulations in 1969 for dealing with supposedly ineffective drugs which, in short, give manufacturers the right to a public hearing only if they can provide substantial scientific evidence to show that their product is effective.

In one specific case, a drug called Lutrexin, the FDA ordered the manufacturer to withdraw the drug from the market because the NAS had found it to be only "possibly effective". The manufacturer asked for a public hearing, which the FDA denied, and the case then went to a court of appeals. The upshot was that the court ruled against the FDA, and the FDA subsequently appealed against the decision to the Supreme Court on the grounds that it would require the agency to hold public hearings on nearly all its cancellation orders. The FDA argues that such a requirement would cause a massive log-jam and effectively block the whole programme.

Another case being reviewed by the Supreme Court involves the question of whether the FDA has the power to ban from the market drugs which it has not specifically approved in the first place—the so-called "me too" drugs. Such drugs were allowed on the market without specific FDA approval because they have identical, or very similar, formulae to other drugs which had already been approved as safe.

The FDA claims that if it orders one drug off the market because it is ineffective, all others with the same formula should meet the same fate. A court in Philadelphia backed the FDA last year, but a court in Virginia ruled that federal courts and not the FDA must decide whether "me too" drugs could be ordered off the market under the 1962 amendment. This is another of the nuts the Supreme Court must attempt to crack.

OBSESITY

Amphetamine Ban

IN spite of legal challenges to its procedures for ordering ineffective drugs off the market, the Food and Drug Administration has decided to stop the sale of anti-obesity products which combine amphetamines with other drugs. The agency believes that such combinations are no more effective than amphetamines alone, and has already taken steps to prevent their manufacture by asking the Department of Justice to eliminate production quotas for the products. Last month, the FDA warned that even amphetamines alone are of only marginal value in the treatment of obesity, and several witnesses before the Senate Select Committee on Nutrition and Human Needs urged that the combination drugs should be banned (see *Nature*, 240, 436; 1972).

The Biggest World Model So Far

from our Special Correspondent

Geneva, January

THE World Employment Programme on which the International Labour Office is likely to have spent \$2 million in the four years to 1975 has had the somewhat unexpected result of having thrown up a dynamic computer model comparable in many ways to the Meadows model, developed for the Club of Rome at MIT, but which is at once much larger and more sophisticated. The model is being used to test the interactions between population growth and employment and is intended to throw light on questions of employment policy in developing countries. It is called *Bachue-1* after the Colombian goddess of love and fertility and harmony between nature and man.

Some idea of how ambitious the simulation model is can be gathered from the fact that even the refined version of it occupies more than 600 K of storage space on the IBM 370 computer installed at the International Computing Centre in Geneva. The first run of the model a few weeks ago occupied no fewer than 796 K of the 1,000 K core, leaving precious little room for the operational programs necessary to allow the computer to function properly. The work at the ILO has been directed by Dr Richard Blandy, an Australian economist who has just returned to Flinders University, Adelaide. Day to day responsibility for the development of the system has been shouldered by Dr René Wéry, with Mr Ray Curnow of the Science Policy Unit, University of Sussex, acting as a consultant.

Everybody concerned with the project is full of admiration for Professor Jay Forrester's DYNAMO Language in which the programs have been written and especially for the flexibility which this permits and for what are called the "debugging facilities" which, among other things, allow the system of programs to make explicit assumptions about the values of particular parameters when these have been incompletely specified. But DYNAMO Language is extravagant in its use of storage space, so that there are plans—or hopes—to rewrite the lot in Fortran.

The simulation model differs from the Meadows model in the degree to which the variables are disaggregated. Population, for example, is represented by separate variables for each interval of five years between birth and old age, and these categories are further subdivided by sex and residence in urban or rural areas. In the version of the

model now functioning, fertility (or, more specifically, the number of births each year to women of different ages and in different categories) is reckoned to be a function of child mortality (and positively correlated with it), the education of adults, the opportunities for women to work in modern jobs, the numbers of unpaid family workers in the labour force, the education of children and the average income of households. The model also makes possible a variety of assumptions about the effectiveness of family planning. One of the innovations built into *Bachue-1* is the possibility of simulating the migration of workers from country to town.

Compared with previous models, the ILO simulation breaks new ground in its detailed description of the labour force, but its authors acknowledge that data from developing countries which purport, for example, to indicate the proportion of the female population in work of various types are probably highly suspect. Data to represent the several categories by which the educational system is described are probably easier to come by but the assumptions which have been made about the social and economic impact of different kinds of education are probably among the most difficult of all to test. The economic parts of the model are impressively disaggregated—with the problems of developing countries in mind, the ILO team has distinguished between modern and traditional agriculture, modern and traditional manufacturing industry and employment in service functions.

It goes without saying that the ILO model differs from the Meadows model in that it makes no allowance for such things as the effect of pollution on mortality nor of the availability of natural resources other than food. For that matter, the model also fails to allow for differences of caste or race within countries.

The immediate objective is to use the model as a means of telling which kinds of development in a developing country are most likely to help shrink the size of the labour force without work or engaged in non-productive tasks. To provide some yardstick of what will happen, the modellers have used data from countries in Latin America between 1960 and 1970 to provide a starting point for a forty year simulation. They acknowledge that a great deal of work remains to be done before the model can be regarded as mathematically self-coherent and stable to transients, and before the assumptions which have been built into them can be tested; some will no doubt be validated,

others will need further refinement.

What is likely to be the value of this computer model? Although the original incentive for its development was the haunting unemployment problem in developing countries, the model is plainly able to throw light on a wide range of problems concerned with development. One possibility, for example, is that the model may throw light on the economic consequences of rural-urban migration. With the assumptions so far made, such demographic movements may have the effect, for all their immediate or at least conspicuous ill-effects, of making possible a more rapid growth of family incomes. With the assumptions so far incorporated, the model also suggests that an immediate reduction of fertility will not bring social and economic benefits within a decade or even two, although it must be acknowledged that the cost of capital investment in education and health care may not be adequately catered for in the model.

This is why one of the most attractive features of this ambitious exercise in computer simulation is that the modelling is supported by a vast programme of field studies in which attempts will be made to gather from developing countries some of the data on which refined assumptions may be formulated. Unlike some others, the ILO's model is a starting point for a programme of social and economic research which is certain to be made more pointed by the preliminary results of the simulations, but which is a sufficient objective in itself.

Those responsible for the computer modelling hope that in the months ahead it will also be possible to use the model as a means of generating hypotheses for other parts of the World Employment Programme. One of the most stimulating of these is the attempt which is being made to assess the impact of new technologies on employment in developing countries. Should a growing nation invest its capital in agriculture, in advanced technology such as nuclear power or in something half way in between? Several field studies have already been commissioned of the operation of modest industrial enterprises, canning factories for example, on the social condition of the communities in which they are placed in countries such as Kenya. In due course it may be possible to develop yardsticks by means of which developing countries may decide what kinds of new developments will be best. Then will come the task of persuading aid-giving countries that they should be backed with hard external currencies.

NEWS AND VIEWS

Problems of Understanding Pulsars

PROGRESS in pulsar theory is slower than might be expected from the wealth of observational data now available. An important step forward was the statement by Goldreich and Julian in 1969 (*Astrophys. J.*, **157**, 869) that a rotating, magnetized neutron star must be surrounded by a very remarkable co-rotating magnetosphere, in which gravity counts for almost nothing and only electrodynamic forces determine the particle motion out to a distance where the co-rotation velocity approaches the velocity of light. Mestel (*Nature Physical Science*, **233**, 149; 1971) has recently given a more general account of this theory. But astronomers seem to be no nearer to an understanding of the basic radiation process. If anybody claims to understand pulsars, let him answer the question "How do they pulse?"

What is needed is a twin attack from observation and from theory. A co-rotating magnetosphere provides a fairly straightforward analytical problem, provided that the particle density is so low that it does not affect the magnetic field. This is almost certainly not the case in the interesting region close to the velocity of light circle, but there are other problems extending throughout a more realistic magnetosphere. A self-consistent solution including a full complement of particles and magnetic field must face problems such as the following.

- Do the particles gain and radiate most of their energy close to the pulsar surface or near the velocity of light circle?
- What are the particle trajectories near the velocity of light circle?
- Is there a complete charge separation in clearly defined regions of the magnetosphere?
- Is the charge of the outward electron flow balanced by a return flow of electrons or by an outward flow of protons?
- What is the mechanism for bunching which leads to coherent radio emission?

Observation may provide some help in describing the geometry of the field and the electron trajectories in the emitting region. But to date there is no agreement about the location of the emitting region, and no clear interpretation of the polarization measurements which should provide a description of the magnetic field. There should be some critical experiments which could provide a real guide to the field configuration, but none has been forthcoming so far.

Another approach is to consider how the pulsar magnetosphere might evolve as the spin rate decreases. An interesting way out of the analytical problem then emerges: old pulsars might not have a dense magnetosphere at all. Ruderman (*Phys. Rev. Lett.*, **27**, 1306; 1971) has suggested that slow rotation provides such a small surface field that no electrons are emitted. Thermionic emission is unlikely, so the magnetosphere may be empty. This would certainly account for the dearth of pulsars with periods greater than 2 s.

Endean carries this argument further in an article on

page 184 of this issue of *Nature* by attempting to put the observational data on young and old pulsars into some sort of evolutionary sequence. First attempts at this will undoubtedly be shot down by observers who are in possession of so much detailed material that they can shoot down almost any interpretation. But the chief point is that a theorist who has contributed to the analysis of the magnetospheric problem is now in touch with the observations. There is a particular need for an explanation of the pattern of drifting sub-pulses, which seem to be characteristic of the older pulsars. Endean points out that this may fit the evolutionary stage just before the magnetosphere becomes a vacuum. It also fits the hypothesis that the emission takes place from close to the velocity of light cylinder.

There is still some way to go before speculation of this kind can give way to a solid theory of the pulsar magnetosphere, but it is encouraging to see speculation which takes some account of the observational facts.—F. G. S.

Miocene-Pliocene Boundary

Fiji is odd, for it contains both continental and oceanic rocks, but the concept of world-wide stratigraphical boundaries is even odder. Both are, however, critical to an understanding of the evolution of the Earth and are brought together by Gill and McDougall on page 176 of this issue of *Nature*. The oddity of stratigraphical boundaries is that they are purely conceptual phenomena, originally used in the eighteenth century to distinguish and relate one sequence of rocks to another. As such they usually reflected some particular facies of sedimentation, the boundaries being marked by either a significant change in the nature of the rocks deposited or, more commonly, by a distinct break in sedimentation during which the underlying rocks were often tilted or folded and then partially eroded before deposition of the next sequence.

Clearly, the recognition of such lithostratigraphical intervals is essential for unravelling the geological history of any one area, but problems arise when attempts are made to trace such boundaries into neighbouring areas. This is because the lithological unit clearly represents a specific interval of geological time during which that particular rock type was deposited whereas the boundaries also represent periods of geological time. Thus the boundaries of such units may, if they are recognizable at all, occur at significantly different times in closely neighbouring areas.

During the Phanerozoic, the past 550 million years, there was a great increase in the types and preservability of organisms. This has allowed the definition of time intervals at that time during which certain organisms lived. In 1810 Brongniart defined the Tertiary period as that interval during which the rocks overlying the Cretaceous Chalk of the Paris Basin were laid down; this is the

period now referred to in elementary textbooks as the Age of Mammals. This interval was subdivided by Sir Charles Lyell in 1833, using the studies of Deshayes, into four intervals, of which the Miocene was equivalent to the Middle Tertiary and was the time when rocks were deposited which contain between 20 and 40 per cent fossil molluscs of species existing today, and was therefore less (*mio*) recent (*cene*) than the Pliocene (more recent) rocks, which contain more than 50 per cent of species still living.

The problem with this form of time division is that a large number of species must be identified in order to perform the statistics. More often, therefore, a "faunizone" is recognized—a stratigraphical unit of rocks containing a specific assemblage of organisms of which a few particular fossils are usually taken as indices. The recognition of these index fossils is therefore sufficient to locate the rocks within an established biostratigraphical time sequence. Such divisions can obviously be synchronous where the organisms concerned are widespread and when new diagnostic species appear simultaneously on a world scale, but such requirements are difficult to meet because of the differing ecological restrictions on organisms. Different zonations thus grew up based on terrestrial fauna, shallow marine fauna and deep-water fauna.

The advent, principally in the 1960s, of precise dating using the radioactive decay of potassium to argon meant that the different faunal or floral divisions of geological time could be measured against an absolute time scale and that the biostratigraphical zonations could therefore be correlated with each other. The precision of dating, then about ± 5 per cent, was, however, still poor, and has now improved to $\pm 1-3$ per cent where suitable rocks can be found to date. This has meant that Evernden *et al.* (*Amer. J. Sci.*, **262**, 145; 1964) were able to obtain provisional dates for terrestrial faunal zones in the western United States from interbedded lavas, but many Tertiary zones have only been dated on minerals extracted from tuffs or using glauconite.

On a world scale, the most useful and widespread faunal zonation is based on planktonic Foraminifera, for these microorganisms occur in all marine sequences, and components of the fauna evolve rapidly. Micropalaeontologists, such as Bandy, Berggren, Blow and Bolli, have recognized forty-one biostratigraphical zones in the Tertiary and two in the Quaternary (Berggren, *Nature*, **224**, 1072; 1969), although there are some difficulties in correlating between equatorial and temperate latitudes. Using this system the beginning of the Miocene is marked by the first appearance of the genus *Globigerinoides* and Cita and Blow (*Riv. Ital. Palaeontol.*, **75**, 549; 1969) have related the micropalaeontological succession of southern Italy with the tropical zonation scheme. The absolute age of the boundary was, however, still determined by the dating of the Mt Capanne granodiorite of Elba at 7 million years, as fragments of this intrusive were found in undoubted Lower Pliocene rocks of the Italian mainland. This date alone was sufficient to upset many previous estimates, which had hitherto been more than 9 million years and mostly around 12 million years. Such an age had been consistent with the definition of the Pliocene by vertebrate palaeontologists who, since Matthew (*Bull. Amer. Mus. Nat. Hist.*, **50**, 59; 1929), have considered the appearance of the three-toed horse *Hipparion* to be simultaneous in North America, India and Europe and to define the start

of the Pliocene. Radioactive dating of rocks in Eastern Europe and the Mediterranean suggests that *Hipparion* appeared in these regions some 11 to 13 million years ago. Such an event must be considered of parochial importance by comparison with foraminiferal zonation or even by comparison with the later regression of the sea from the Mediterranean, associated with major evaporite formations, although sedimentation continued in a few areas such as Sicily and in the Pannonian Basin of south-east Europe. It is in Sicily that the Miocene/Pliocene boundary is now defined within the "*trubi*" sequence, above the Messinian marls and gypsum.

This boundary, however, lies within the planktonic foraminiferal N18 zone, for *Sphaeroidinella dehiscens*, which marks the base of zone N19, appears a little later and the boundary is straddled by *Globigerina nepenthes*, *Globorotalia tumida*, *G. miocenica*, *G. multicamerata* and other Foraminifera of the N18 zone. Unfortunately the marls and gypsums do not afford material which can be dated radiometrically. But in the south-west Pacific, particularly in New Zealand and Fiji, the foraminiferal zones can be identified and also dated from interbedded lavas and tuffs. This has led Lienert, Christoffel and Vella (*Earth Planet. Sci. Lett.*, **16**, 195; 1972) to propose a date of 4.6 million years in New Zealand; Gill and McDougall now put the boundary at 4.9 million years in Fiji.

The final test, however, has not yet been carried out, for reversals of the polarity of the geomagnetic field occur instantaneously on a world-wide scale (within some 10,000 years) and polarity changes have been delineated at 4.33, 4.41, 4.48, 4.65 and 5.10 million years (Opdyke, *Rev. Geophys. Space Phys.*, **10**, 213; 1972), and although both antipodean articles attempt to fit their data within this geomagnetic time scale, their information is, as yet, inadequate.—D. H. T.

Attacking Palindromic DNA

ONE of the most intriguing of the systems which *Escherichia coli* utilizes to protect itself against foreign DNA is the host modification and restriction system. Each strain of *E. coli* possesses its own particular system for imposing on its DNA a characteristic pattern of methylation. This modification takes place when the DNA is replicated, and any DNA entering the cell—for example a phage genome—which lacks the appropriate modification pattern is degraded by an endonuclease activity of the same system. The genetic characteristics of mutants in the system imply that one enzyme complex may both modify and degrade in appropriate circumstances. One of the most interesting questions about these systems is what confers the basis for their specificity, and no less than six articles in the November issue of the *Proceedings of the National Academy of Sciences* are devoted to this question.

Four of the articles concern one of the enzyme systems specified by the RTF (resistance transfer factor) of *E. coli*, which codes for the enzymes RI and RII. Enzyme RI converts the circular DNA of Simian Virus 40 to a linear form; enzyme RII introduces multiple breaks into this alien nucleic acid. Mulder and Delius (*Proc. US Nat. Acad. Sci.*, **69**, 3215; 1972) have used

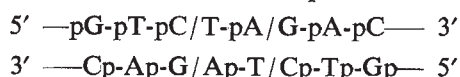
partial denaturation mapping to locate the site(s) of the break in the circular SV40 DNA. After linear molecules have been partially denatured by alkali, the sequences rich in guanine and cytosine which remain double stranded can be distinguished in the electron microscope from the denatured single strands. By using this ingenious technique, Delius and Mulder found that the entire population of linear molecules generates essentially the same denaturation map. This implies that there is only one site at which the enzyme cleaves the circle to produce linear species.

Another way to denature SV40 DNA is to treat it with the gene 32 unwinding protein specified by phage T4, which denatures only one site in the genome. The distance of this site from the end can be measured in linear molecules, and Morrow and Berg (*ibid.*, 3365) show that it is constant in the linear SV40 DNA molecules produced by the RI restriction endonuclease. Morrow and Berg were also able to generate fixed points on the SV40 DNA by hybridizing it with the DNA of *Ad2+ND1*, a hybrid between adenovirus and SV40. The region of SV40 DNA homologous to the hybrid is also always located a constant distance from the ends of linear molecules. These experiments located the cleavage site somewhere in the late gene region of SV40.

The linear molecules resulting from treatment with RI endonuclease are of more than academic interest, for Jackson and Berg (*ibid.*, 2904) have already used them to prepare circular SV40 DNA molecules which contain several of the genes of phage lambda and the galactose operon of *E. coli*. Mertz and Davis (*ibid.*, 3370), in an article communicated by Berg, now report that the linear SV40 DNA can be converted to closed circles by incubation with the enzyme polynucleotide ligase of *E. coli*. Both linear and circular oligomers are generated in addition to the closed monomeric circles.

The implication of this observation is that the restriction enzyme makes a break in each strand of the double helix at a different point, so that when the linear molecule is formed each end of the duplex has a protruding length of single strand. Because the single stranded regions at each end are complementary, they can hydrogen bond together to reform the circle and can then be covalently linked by ligase. By measuring the equilibrium constant for the joining reaction at different temperatures, Mertz and Davis deduce from the enthalpy of reaction that about 5 ± 1 nucleotides must separate the two points at which the enzyme makes its breaks.

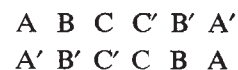
One way for an enzyme to cleave both strands of DNA is for the recognition sites to be palindromic, so that the action of the enzyme is symmetrical with respect to the DNA base pair sequence. By studying the products of the reaction of the enzyme of *Haemophilus influenzae*, Kelly and Smith showed some time ago (*J. Mol. Biol.*, **51**, 393; 1970) that the enzyme cleaves both strands in the centre of the palindromic sequence:



Danna and Nathans (*Proc. US Nat. Acad. Sci.*, **68**, 2913; 1971) have shown that this enzyme cleaves SV40 DNA into eleven specific fragments, which may prove useful for sequencing studies.

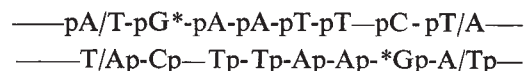
If the action of the RI endonuclease on SV40 DNA

also takes place at a palindromic site, the "heads" and "tails" of the linear molecules created by cleavage should possess the same terminal single stranded sequence. Renaturation and treatment with ligase should then yield molecules in which heads or tails have been joined to each other instead of to their original partner sequence. Mertz and Davis point out that such molecules should "snap back" instantly to form hairpin duplexes. Their observation of such structures supports this model, which demands that the enzyme must have a two-fold axis of symmetry so that it cleaves the two strands of DNA at opposite ends of a sequence such as:



so that it must generate even numbered breaks. By comparing the number of cleavages made in different DNA substrates with the number of times any particular length of bases would occur by chance, Mertz and Davis show that the recognition site is probably six base pairs long, in which case it will occur once in every 4,100 base pairs. If the same sequence is recognized in all DNAs upon which the enzyme acts, then any two products of the reaction should join together by renaturation and treatment with ligase. These experiments therefore offer the prospect that DNAs of different types can be covalently connected into hybrid molecules.

In another article in the same issue of the *Proceedings*, Hedgpeth, Goodman and Boyer (page 3448) show by sequence studies of the termini released by the enzyme that its substrate is indeed the predicted palindromic sequence of DNA. In this case, the ambiguities in the sequence lie outside the sites of cleavage, instead of at the centre as with the *H. influenzae* recognition site. The enzyme cleaves at the asterisked points two bases from each end of the sequence:



generating the cohesive ends predicted from the properties of the linear products.

The specificity of action of the RI enzyme bodes well for sequence studies on its substrates, but Horiuchi and Zinder (*ibid.*, 3220) show that the enzyme of *E. coli* B is not so selective. Phage f1 is a useful substrate for this enzyme because genetic analysis identifies two sites at which mutation can confer resistance to its degradative action. But although the enzyme does not attack phage which is mutated or methylated at these two sites, when the template is in a suitable state it appears to be cleaved at several sites. This implies that the enzyme recognizes only two sites, but that cleavage at many other sites may succeed recognition of a susceptible template. The elegant studies on the *H. influenzae* and RI endonucleases may not, therefore, apply to all of the restriction enzymes.

Turning to the modification activity of the system of *E. coli* K, Haberman, Heywood and Meselson (*ibid.*, 3138) show that the same enzyme preparation can both methylate and restrict; only S-adenosyl-methionine is needed for modification activity *in vitro*, but degradation can take place if Mg^{2+} ions and ATP are also supplied. The enzyme is a multimer and in both *E. coli* K and B it seems that in addition to this large enzyme with both methylated and restricting activities, a smaller enzyme

can be found which has only the modification capacity. This prompts the speculation that it consists of the recognition and modification elements of the enzyme, but lacks the component with restriction activity. These activities are just those which are predicted by a model for the enzymatic action derived from the genetic studies of Hubacek and Glover (*J. Mol. Biol.*, **50**, 111; 1970).—From our Molecular Genetics Correspondent.

MYOSIN

Casting Vote

from our Molecular Biology Correspondent

It is a familiar feature in the recent history of research on muscle proteins that experiments devised to determine whether the stoichiometry of an interaction is one or two have generally given answers around 1.5. According then to whether the disposition of the authors has been to simplify or complicate the picture by some new proposition (that the myosin heads are not equivalent, for example), a choice of interpretation has presented itself. A prominent example is the question of whether or not both heads are capable simultaneously of binding actin. By the democratic criterion at least, the issue has been open, but Eisenberg, Dobkin and Kielley (*Biochemistry*, **11**, 4657; 1972) have now weighed in heavily in favour of the simpler answer. Their technique was to titrate polymerized actin (F-actin) with heavy meromyosin (which by virtue of its truncated shaft has more tractable solubility properties than myosin itself), and to measure the fraction remaining in the supernatant during sedimentation in the analytical ultracentrifuge, using ultraviolet scanner optics. It is important to take account in such experiments of a background of unpolymerized actin, the magnitude of which is more or less independent of the total actin concentration, and therefore probably represents a "critical" (equilibrium) concentration of monomer in the highly cooperative aggregation reaction.

The results seem to leave little room for doubt: all the heavy meromyosin is carried down with the F-actin up to a ratio of one myosin molecule to two actin monomers, after which all further myosin remains in the supernatant. The binding constant is too high to measure, regardless of ionic strength, as long as ATP is absent. The results, although agreeing with those of other recent studies, are in irreconcilable and unexplained conflict with the conclusions reached a few years ago by

Young, on the basis of an essentially identical experiment. They undermine still further the rather shaky structure of evidence regarding functional differences between the two heads of the myosin molecule.

Another result that seems to go some way towards resolving an irksome ambiguity is that of Malik, Marchioli and Martonosi (*Arch. Biochem.*, **153**, 147; 1972), who have studied the binding of ADP to heavy meromyosin in the presence of a selection of divalent metal ions. The relevant question is how the demonstration by Taylor and his associates—that the rate-limiting process of the myosin ATPase is the dissociation of the products—is to be reconciled with the reportedly rapid rate of dissociation of ADP from its myosin complex prepared simply by mixing. The answer, it seems, may lie in the difference between the temperatures at which the two studies were done. Malik *et al.* have now performed the ADP binding experiments at 6° C, rather than 25° C, to allow direct comparison with the analyses of the ATPase, and have arrived at the happy conclusion that in these conditions the dissociation of ADP from heavy meromyosin is indeed slow enough to be comparable with the steady-state ATPase velocity.

Malik *et al.* measured first the binding constant of ADP to heavy meromyosin by equilibrium dialysis (showing, it may be remarked, a stoichiometry of just over one ligand per protein molecule, a finding on which they wisely refrain from enlarging). The rate constant of the binding (forward) reaction was measured by stopped flow, taking advantage of the small ultraviolet difference spectrum that the association generates. Dividing this into the equilibrium constant, they obtain an estimate of the reverse (dissociation)

rate constant. Now the binding constant, and also the ATPase activity, vary markedly with the nature of the cation present, yet the correlation of ATPase turnover number with the dissociation rate constant of the ADP-heavy meromyosin complex extends to four of the five cations that were tried, only strontium being somewhat out of line. Both the ADP binding and ATPase level show a correlation with the size of the metal ion, but the two functions peak at different values of the ionic radius.

It is widely recognized that anomalies in enzymatic and stoichiometric properties of heavy meromyosin, as well as of isolated myosin heads (subfragment-1), are apt to arise from proteolytic damage in the course of preparation, because both forms are made by digestion of myosin with trypsin or papain. The inhomogeneity of subfragment-1 preparations is strikingly obvious in SDS-gel electrophoreses shown by Cooke (*Biochem. Biophys. Res. Commun.*, **49**, 1021; 1972), who advocates a new preparative procedure. This involves papain digestion of whole myofibrils, rather than purified myosin. The notion is that the heads themselves are relatively shielded against damage in the fibril, possibly by association with the actin filaments. At all events, after extraction with pyrophosphate and magnesium to dissociate active heads from the actin, followed by gel filtration, a distinctly better looking product results, at least by the electrophoretic criterion. The maximal actin-activated ATPase is also somewhat higher than that of conventional subfragment-1 preparations. On the other hand, the calcium-activated ATPase is appreciably lower than in preparations of the usual type, and this the author tries but fails to explain away. More studies perhaps are needed.

Unidirectional Replication of Mitochondrial DNA

IN *Nature New Biology* next Wednesday (January 24) Kasamatsu and Vinograd report a further step in the exemplary analysis of the mechanism of replication of the covalently closed circular duplex DNA of mouse cell mitochondria, a subject which has been one of the chief preoccupations of Vinograd's group these past two years. Vinograd and his colleagues have shown that the first step in the replication of mitochondrial DNA is the synthesis of an RNA chain some 450 nucleotides long which is complementary to a unique region of the so-called light (L) strand of the mitochondrial DNA. This molecule acts as a primer for the synthesis of the complementary heavy (H) strand once an endonuclease system becomes operative. The nascent daughter H strand displaces the

parental H strand which then acts as a template for the synthesis of a daughter L strand.

What Kasamatsu and Vinograd have now established is that replication of this circular DNA duplex occurs in only one direction and not in both directions around the circular parental molecule. They have done this by examining in the electron microscope the position of the two forks of the replicated region of replicating double length mitochondrial DNA molecules. These molecules consist of two monomeric genomes joined in a head to tail fashion and although two RNA chains are made at the start of their replication only one acts as a template for DNA synthesis. The other simply forms a displacement loop which provides a fixed physical marker.

STEROIDS

Trial of Antiandrogen

from a Correspondent

MANY men, possibly as many as 50 per cent of those over the age of 60, suffer from enlargement of the prostate to the extent that the urethra is constricted leading to urine retention. The usual form of treatment is surgical, but, as an alternative, hormones and hormone antagonists have been used.

It is known that the male sex hormone is involved in the normal growth and functioning of the prostate and is assumed to play a part in the development of benign prostatic hypertrophy. Hormones with antiandrogenic activity have thus been used in treatment, but unfortunately most of these have disadvantages—the use of oestrogens causes feminization and the use of potent synthetic progestins usually leads to a decrease in production of testicular hormones by suppressing gonadotrophin release from the pituitary. One synthetic antiandrogen, cyproterone acetate, seems to have a beneficial effect, but, over a long period, it also seems to decrease spermatogenesis and leads to loss of libido and potency. These effects again probably result from an inhibition of gonadotrophin secretion by the pituitary.

There is also a high incidence of prostatic hyperplasia in dogs. Neri and Monahan now describe (*Invest. Urology*, 10, 123; 1972) the effects of treating dogs with a non-steroidal antiandrogen, 4'-nitro-3'-trifluoromethylisobutyranilide. Sixteen aged dogs were given the compound orally for 6 weeks and a further three dogs were treated with the compound for 1 year at a dose level of 5 mg/kg. The volume of the prostate and the height of the epithelial cells were significantly reduced after treatment for only 6 weeks and these changes were accompanied by a reduction in the secretory function of the prostate as shown by a decrease in acid phosphatase activity and protein content of the gland and by electron microscopic studies. After 6 weeks of treatment the dense secretory bodies, vesicles and the Golgi apparatus, which were seen in the pre-treatment biopsies, were absent showing a loss of secretory function. The testes, as shown by examination of biopsies, appeared unaffected by the treatment and there had been no decrease in the level of plasma testosterone. Within 8 weeks of stopping treatment, the atrophied prostate reverted to a hyperplastic condition.

When given orally, the compound seems to be equally potent as cyproterone acetate, a steroidal antiandrogen which competes with androgens at the target organ receptors. 4'-Nitro-3'-

trifluoromethylisobutyranilide, however, unlike cyproterone acetate, does not have the progestational activity which may be responsible for the pituitary inhibition, neither does it seem to have any other hormonal activity. Furthermore, it does not seem to produce any undesirable side-effects, so that results of trials in humans will be awaited with interest. Even if the results are not so striking in man as in dogs, at least they may lead to some decrease in prostatic size with an improvement in rates of urinary flow.

ENVIRONMENT

Pressure from Pleasure

from our Plant Ecology Correspondent

THE swing in ecological research towards the study of human effects on the environment became evident at a recent symposium of the British Ecological Society in Sheffield (January 2–4). As well as recurrent pollution themes the ecological problems resulting from recreational pressures on the environment received considerable attention and there is evidence that recreational ecology is passing from a descriptive phase (pioneered by Baker in 1935) to an experimental phase.

The current interest in the countryside among the general populace has led to a considerable increase in demand for access to areas which are sensitive to heavy trampling. Dr N. J. Bayfield (Nature Conservancy, Banchory) described how upland footpaths in such areas as the Cairngorms and along the Pennine Way are deteriorating seriously as a result of increasing numbers of

people using them each year. One of the greatest problems is that of lateral spread of footpaths, laying bare additional areas of soil. Dr Bayfield has found that this effect increases with soil wetness, and becomes most severe in the upland bog areas of the Pennines which are particularly sensitive to trampling. Path width is inversely related to roughness of terrain, a fact which suggests that the best sites for paths are the roughest areas.

Some experimental work reported by Dr E. A. G. Duffey (Nature Conservancy, Monk's Wood) indicates that invertebrate animals are even more sensitive to trampling than are most plants. He placed plant litter in nylon bags and laid them in experimental plots on the surface of soil beneath grass. He then subjected them to two levels of trampling (60 and 120 treads per annum) for a year. The effect of the trampling was to fragment the litter and reduce the air space from 63 per cent in the control to 54 per cent and 37 per cent in the two treatments, as measured by thin sectioning of embedded material. The numbers of animals present in twenty-five litter bags were reduced by half and the number of species present by three-quarters, but there were no marked differences between the two treatments. This indicates that animal populations are being severely affected at trampling levels which are too low to affect the composition of vegetation.

Vehicles, of course, can also have a considerable and harmful effect upon vegetation. Dr M. J. Liddle (Bangor) described how vehicle tracks on sand dunes at Aberffraw, Anglesey, first

More of the MS2 RNA Sequence

FIERS and his colleagues at the University of Ghent are continuing remorselessly to what must be their ultimate goal, the determination of the complete sequence of MS2 phage RNA. Having already determined the sequence of the coat protein cistron and part of the replicase subunit cistron, they report in *Nature New Biology* next Wednesday (January 24) the sequence of the codons specifying the last forty-five amino-acids of the assembly protein cistron and the sequence of the intercistronic divide (twenty-six bases) between the assembly protein and coat protein cistrons. Furthermore, Vandekerckhove, Nolf and Van Montag in an accompanying article report the amino-acid sequence at the carboxy terminal end of MS2 assembly protein. This amino-acid sequence is precisely that predicted from the base sequence reported by Fiers *et al.* From their analysis of MS2 RNA, Fiers *et al.* conclude that all sixty-one sense codons of

the genetic code are present in this phage genome albeit at different frequencies.

Also in *Nature New Biology* next week Federoff and Zinder report about the requirement of host cell factors during the replication of Q β RNA and f2 RNA. Federoff and Zinder find that the f2 replicase, a much less stable enzyme than Q β replicase, requires a dissociable host cell factor when it uses both f2 viral RNA and complementary f2 minus strands as template for RNA synthesis. In the absence of this factor the f2 enzyme can, however, use poly (C) as a template for the synthesis of poly (G). Furthermore, the factor required by the f2 replicase seems to be different from those required by phage Q β replicase when it uses Q β viral RNA as template. The precise nature and function of these host factors in the replication of the RNA genomes of the RNA phages remain, however, to be elucidated.

cause the formation of a crust impregnated with organic matter in the surface sand. When this is fragmented by further usage, erosion results. Dr Liddle has experimented with a variety of remedial operations in the early stages of damage and has found that harrowing of the compressed surface accompanied by the addition of fertilizers significantly improves recovery by encouraging the rapid growth of meadow grasses (*Poa pratensis* and *P. annua*).

Experiments of this type may enable descriptive recreational ecology to develop into informed and effective environmental surgery.

PROVIRUSES

Site of Synthesis

from our Cell Biology Correspondent

MOST stocks of most viruses seldom contain more than one infectious particle per ten physical particles and more often contain only one infectious unit per hundred or thousand physical particles. It is therefore almost impossible to determine unambiguously how an infectious virus particle enters a cell and brings about an infectious cycle. One can, however, use the electron microscope coupled with autoradiographic techniques to observe the fate of a sample of the virus particles in a population that is allowed to adsorb to any particular population of cells and assume—this assumption is usually tacit—that the minority of infectious units behave like the majority of physical particles. Dales, Hanafusa and Huima (*Virology*, **50**, 440; 1972) have just done this in a painstaking way using Schmidt Ruppin Rous sarcoma virus and an avian leukosis virus (RAV 2) labelled with ³H-uridine and secondary cultures of chick embryo fibroblasts. Their conclusions, in particular about the site of synthesis of the DNA provirus, are at variance with those of Hatanaka *et al.* who last year reported somewhat similar experiments with murine RNA tumour viruses and mouse cells.

Dales *et al.* allowed the virus particles in the presence of DEAE dextran to adsorb to the cells for 1 h at 4° C or 10° C during which time from 500–2,000 virions irreversibly adsorbed to each cell. Electron micrographs of these cells revealed two interesting points. First, cells comprising about 4–18 per cent of the population which have a distinct morphology different from the majority of fibroblasts adsorb many more virions than typical fibroblasts. Second, the adsorption sites at the surfaces of fibroblasts have a distinct structure; they consist of a reticulum of fibrils and resemble gap junctions.

When cells are warmed from 4° C or 10° C to 37° C, a minority of the virions

are phagocytosed and transported in phagocytic vacuoles to the vicinity of the nucleus and, by 10 min after the temperature shift, labelled RNA molecules can be detected by autoradiography in the nuclei of the cells. The amount of label in the nucleus reaches a maximum about an hour after the temperature shift, and a simple piece of arithmetic based on several not unreasonable assumptions indicates that if all the ³H-uridine remains in viral RNA there must be between twenty-five and seventy-five genome equivalents per infected cell nucleus after an hour at 37° C. This, of course, is only a small fraction of the number of virions that adsorb to the cells. Aware of the problem that viral RNA may be degraded and the ³H-uridine re-utilized in the synthesis of cell RNA, Dales *et al.* performed appropriate control experiments which eliminated this possibility.

Clearly the infection of chick cells by these avian tumour viruses involves the rapid transport of viral RNA to the cell nucleus, and this finding suggests, of course, that the synthesis of a DNA provirus by reverse transcription may well occur in the cell nucleus, which is what one might have predicted. But Hatanaka *et al.* have reported that soon after mouse cells are infected with mouse sarcoma or leukaemia virus, DNA synthesis can be detected by ³H-thymidine autoradiography in the cytoplasm and this synthesis, they suggested, was the synthesis of DNA proviruses. By contrast Dales *et al.* apparently found no evidence of cytoplasmic DNA synthesis above background in chick cells infected with avian viruses. They therefore believe that at least in the avian system proviruses are made within the cell nucleus, not in the cytoplasm.

The complete discrepancy between the conclusion of Dales *et al.* and that of Hatanaka *et al.* merits resolution. The generous way to account for the difference is simply to suggest that the avian and murine systems are quite different, but *a priori* that seems most

unlikely. Perhaps Dales *et al.* could be persuaded to repeat their experiments using murine viruses and cells.

EUTROPHICATION

Is pH the Clue?

from a Correspondent

THE changes in fauna and flora—eutrophication—which result from the addition to lakes or estuaries of large quantities of inorganic nitrates or phosphates from sewage and agricultural run-off are often deleterious, especially to fisheries. The mechanisms of these changes, however, are not always clear. O'Brien and de Noyelles (*Ecology*, **53**, 607; 1973) now show that pH changes may often be implicated.

The initial response to over-fertilization is usually a "bloom" of one or more species of alga. Intense blooms are followed by the disappearance of many animal species, those with high oxygen requirements often seeming to be most susceptible. These disappearances have been ascribed to toxins produced by the algae, to low oxygen concentrations resulting from bacterial decomposition of dead algae, and to high oxygen concentrations caused by rapid photosynthesis.

O'Brien and de Noyelles applied one of three fertilization treatments to each of six experimental ponds of equal size, with two remaining as unfertilized controls. For two summers, various environmental parameters were monitored, together with the densities of plant and animal species. In the ponds with the most fertilizer, summer increases in phytoplankton were followed by disappearances of crustacean zooplankton. These disappearances occurred at different plant densities but at almost identically high pH values. Furthermore, water from these ponds could be detoxified by 'Millipore' filtration, which caused a slight reduction in pH with no change in oxygen tension.

The authors present further evidence which, though not conclusive, suggests

Nuclear Giant RNA Contains Messenger Sequences

THERE has recently been a great deal of indirect evidence that heterogeneous nuclear RNA is a precursor of mRNA. Direct proof of this is provided in last Wednesday's edition of *Nature New Biology* (**241**, 66; 1973) by Williamson, Drewienkiewicz and Paul, of the Beatson Institute of Cancer Research, Glasgow. These authors report that when heterogeneous RNA with a high molecular weight from mouse reticulocytes was injected into *Xenopus* oocytes along with labelled histidine, and the soluble fraction was then analysed on a 'Sephadex G-100' column, radioactivity

was located in the haemoglobin region. Material from this region co-chromatographed with mouse globin chains when examined on CM cellulose. Similar labelled material from a control experiment did not chromatograph as globin. When brain heterogeneous RNA, isolated in the presence of a reticulocyte lysate, was injected into *Xenopus* oocytes no globin synthesis occurred.

These results eliminate the possibility that aggregation of heterogeneous RNA and cytoplasmic globin mRNA had occurred during the preparation of the heterogeneous RNA fractions.

that a pH effect may underlie disappearance of zooplankton from "culturally enriched" waters of low buffering capacity. This disappearance would not only disrupt food chains leading to higher trophic levels (for example, fish), but would also remove predation on the algae and lead to further increases in algal density.

It would seem, though the authors do not suggest it, that if this effect is confirmed there would be circumstances in which pH monitoring with occasional slight manipulation could prove worthwhile "preventive medicine".

LIQUID HELIUM

No a.c. Josephson Effect

from a Correspondent

THE implication of some recent work by Musinski and Douglass (*Phys. Rev. Lett.*, **23**, 1541; 1972) is that many earlier reports which purported to describe investigations of the a.c. Josephson effect in liquid helium were in fact describing nothing of the kind. The a.c. Josephson effect is an important macroscopic quantum phenomenon predicted to occur in superfluids.

The only superfluids known are liquid ⁴He and the electron gas in a superconducting metal, so there has naturally been a considerable incentive to compare these two systems: the analogues of phenomena observed in superconductors have been assiduously sought in liquid helium, and *vice versa*. A basic feature of superfluids is that they display long range order, that is the motions of individual particles throughout a given volume of a superfluid are very highly correlated, by contrast with the random motions of classical kinetic theory. A particularly interesting situation arises when two such volumes are weakly coupled through a very narrow channel, so that neither perfect correlation between their particle motions, nor complete independence, can be assured. Josephson predicted that if there is also a difference in chemical potential (an energy per particle) between the two volumes, then periodic flow should occur in the connecting channel with a frequency proportional to the chemical potential difference. This remarkable assertion was soon verified experimentally for superconductors, and the phenomenon has subsequently been investigated in great detail. It was always anticipated that the effect would be much harder to find in helium (chiefly because helium atoms are neutral and much heavier than electrons), so the report by Richards and Anderson (*Phys. Rev. Lett.*, **14**, 540; 1965) that they had seen the a.c. Josephson effect in superfluid helium was hailed as a considerable scientific achievement.

Richards and Anderson inverted the experiment which had originally been proposed. They coupled two baths of helium through a narrow (1.5×10^{-3} cm) orifice, forced periodic flow within it by using a beam of ultrasound, and argued that anomalies should then be seen in the relative chemical potentials of the two baths. What they actually observed was that particular values of the difference in liquid level between the two baths, corresponding to differences in their chemical potentials, remained stable for long periods of time: liquid did not flow through the orifice to equalize the levels, as would be expected classically. The stable level differences were equally spaced from each other, in agreement with theory, and the effect ceased as soon as the ultrasonic transducer was switched off. After the experiment had been improved and repeated in several different laboratories the phenomenon seemed to be well authenticated, although, as Richards himself remarked in a later report, the stable states were always disturbingly hard to find.

Musinski and Douglass have now repeated the experiment yet again, but with two extra parameters at their disposal. They were able to create a small variable temperature difference between the two baths which, like a level difference, changes their relative chemical potentials. They also used an ultrasonic transducer whose frequency

could be varied. If the values of level difference where arrest of flow occurred are really determined by the a.c. Josephson effect, then they should be changed by variation of either temperature difference or frequency. In the event, Musinski and Douglass found that the level differences were almost independent of temperature difference, and that their frequency dependence was in the wrong direction. Moreover, they also noticed that the stable states were occurring at particular absolute heights in one of the baths, and were not dependent on the level difference between the two baths. The inescapable conclusion is that the stable states of zero flow observed by Musinski and Douglass were not Josephson states, but artefacts arising from the experimental arrangements. It therefore seems highly probable that Richards and Anderson, and also the several other workers who came after them, misinterpreted their experimental data and thus did not, as they believed, observe the a.c. Josephson effect. If this is the case, it must come as a considerable disappointment to all scientists interested in superfluidity.

But why did earlier workers not apply the simple tests used by Musinski and Douglass to confirm the interpretation which they were placing on their data? Why has it taken seven years to show that their conclusions were probably erroneous? The answers

Background Radiation no longer Anomalous

FIVE years ago cosmologists were concerned to read in *Physical Review Letters* of the anomalously high values that emerged from the first far infrared measurements of the background radiation. These cast doubt on the role of the background radiation as a cornerstone in the evidence of a big-bang cosmology.

The cosmologists can now breathe again. In November last year the group responsible for the original measurements reported a repeat experiment in which no unusual flux was detected (Houck, Soifer, Harwit and Pipher, *Astrophys. J. Lett.*, **178**, L29; 1972), and next week's *Nature Physical Science* (January 22) contains a confirmatory report by a group at the Los Alamos Scientific Laboratory of the University of California.

The anomalous flux came to light in a rocket flight to discover whether the 2.7 K Planck curve describing the background radiation at radio wavelengths also applies in the submillimetre region where the blanketing effects of the atmosphere make observations difficult. It came as a surprise that the measurements showed a flux corresponding to a blackbody temperature between 4 and

6 K higher than the expected 2.7 K. Although it was almost immediately pointed out that this result is in conflict with some upper limits to the strength of the background radiation which can be obtained from studies of optical absorption lines in interstellar material, cosmologists will be relieved that rocket experiments are failing to reproduce the original findings.

The experiment carried out by the group responsible for the original result involved a telescope based on the original instrument, but with some modifications to rule out possible sources of interference which had been suggested to the authors, and the result was consistent with a 2.7 K background. This is also the result of the Los Alamos experiment, although in this case the analysis was complicated by problems encountered during the rocket flight.

There is still no explanation for the earlier anomalous flux, however. It now seems that it cannot have been of galactic or cosmic origin; the question is whether it was a consequence of some deficiency in the measuring instruments, or whether some phenomenon, possibly linked with the solar cycle, can be implicated.

seem to be that anybody who performs an experiment with the clear expectation of a certain result, and then obtains that result, can only too easily fall into the trap of being less than fastidious in eliminating ambiguities of interpretation. The moral, for all scientists, is obvious.

MOLECULAR LIQUIDS

Excitons Found?

from our Molecular Physics Correspondent

ONE of the more notable received ideas to be dispelled in recent years, as far as the physics of condensed phases is concerned, is that the existence of localized electronic excitations, or "excitons", is an essentially solid state phenomenon conditional upon the regularity and symmetry of the crystal lattice. It was not until 1966 that Rice and Jortner (*J. Chem. Phys.*, **44**, 4470; 1966) pointed out the obvious, namely that, with respect to distances comparable with the wavelength of the exciting radiation, simple liquids also show the required translational symmetry—in the precise technical sense that a quasi-free electron may be represented as a plane wave to an accuracy effectively unimpaired by the fluctuations in liquid structure over distances of a few atomic radii.

Almost simultaneously, Beaglehole in Chicago (*Phys. Rev. Lett.*, **15**, 551; 1965) obtained spectra from liquid xenon which were consistent with the existence of Wannier-type excitons, those with relatively large electron-hole separation and correspondingly weak binding. Since then the experimental side of the field seems to have been relatively static, although a burst of considerable energy was registered as Rice, Jortner and colleagues began to extend their original theoretical insights. (See, for example, Rice, Nicolos and Jortner, *J. Chem. Phys.*, **48**, 2484; 1968; Fisher and Rice, *Phys. Rev.*, **176**, 409; 1968.)

A new paper now raises the question of whether excitons can be observed in molecular, as opposed to atomic, liquids. Eckhardt and Nichols (*Phys. Rev. Lett.*, **29**, 1221; 1972) of the University of Nebraska describe measurements of the specular reflexion spectrum of pure liquid α -methylnaphthalene, in the region 215 to 400 nm, which show both the broadening and redshift, relative to the corresponding spectrum in solution, characteristic of exciton formation. The comparison is not straightforward, however, because the opacity of the neat liquid in the required region is far too high for a detailed spectrum to be observed directly in absorption—hence the need to rely on reflexion spectra from the liquid surface. To effect the comparison it is then necessary to perform a Kramers-Krönig transform on

the results, thus extracting the required imaginary part of the refractive index, which corresponds to the absorption.

The authors are cautious in their ascription of the results to exciton formation, pointing out that dielectric effects in the solutions used might lead to the observed shifts and broadening, although this seems somewhat unlikely.

Nevertheless, these doubts about interpretation reflect the extent of the difficulty of establishing real contact between quantum mechanical theories of the exciton phenomenon and the optical effects which can be reliably observed. Although valuable progress has been made by the Chicago school in relating exciton scattering, and hence lifetimes and band-broadening, to simple models for the interaction in the liquid, several largely independent difficulties—for example, uncertainty about the detailed nature of small-wavelength fluctuations in the liquid, and lack of information about the distribution of excited states in even the simplest substances—combine to frustrate most attempts to calculate quantitative broadenings and shifts from fundamental quantum statistical mechanics. The advances made are nonetheless considerable, for they have established the general interrelationships between the phenomenon and other questions of vital interest such as the dynamics of polarization waves and the characteristics of translational collective modes; the latter were introduced into the more general context of liquid theory by Zwanzig (*Phys. Rev.*, **156**, 190; 1967).

For the time being the outlook would seem to favour the view, recommended by Rice, that once liquid exciton formation is established it should be regarded as a particularly sensitive optical probe complementary to others now available for deriving information about time-

dependent liquid structure, rather than as a problem demanding immediate elucidation through imperfect models of the liquid state. Of course, the distinction is not clear, and it is precisely for this reason that the problem is fascinating; to an extent rather unusual among similar problems, the whole verification of the construction placed upon the experimental results, and not simply their direct explanation, is at stake in the search for an adequate theoretical model. The resulting mix of experimental and theoretical difficulties is, at all events, extraordinarily rich, made up of unsolved, yet promising, problems in spectroscopy, N body theory, and dielectric phenomena, to name only the principal ingredients.

SEISMOLOGY

SHOAL versus Fallon

from our Geomagnetism Correspondent

ALTHOUGH attempts to discriminate between earthquakes and underground nuclear explosions were given significant stimulus and emphasis by the political events of the 1960s, they were necessarily firmly rooted in academic geophysics. In recent years, however, published comparisons of the effects of explosions and earthquakes seem to have acquired an altogether more explicitly academic flavour. Clearly the two aims are far from mutually exclusive; but it is perhaps indicative of a change in emphasis that in a new study of elastic wave radiation from the two types of event Lambert *et al.* (*Geophys. J.*, **29**, 403; 1972) barely mention discrimination. Indeed, they are far more interested in "information about the intrinsic character of the source and the propagation medium" and parameters "of fundamental geophysical interest".

Methane and Carbide on the Moon

IN next Monday's *Nature Physical Science* (January 22), Cadogan, Eglinton, Maxwell and Pillinger report measurements of the methane and carbide contents of Apollo 11 fines. Cadogan *et al.* took samples containing sizes between 108 and 152 μm and separated them into six fractions with differing densities and magnetic susceptibilities. They find that both methane and carbide occur preferentially in those fractions which contain strongly magnetic particles, and they go on to explain this in terms of the exposure and impact histories of the individual grains.

Previous work by this Bristol group has shown that the contents of methane and carbide in lunar fines are closely related to the time for which the fines have been exposed on the lunar surface,

and also that both compounds seem to reside chiefly on the surfaces of the particles. Their new measurements cause them to come out against one of the possible sources of carbide—an impact vapour cloud rich in iron and carbon. If much of the carbide in the fines were formed in this way, they say, then all particles should have a film of iron and carbide on them, and should thus be magnetic and contain comparable quantities of carbide. That is not the case.

On the other hand Cadogan *et al.* find in favour of carbide generation either by meteorite impact or solar wind implantation. Iron silicates could be reduced to iron under these circumstances and carbon already present in the particle could then be dissolved to form carbide.

To obtain this information and estimate the relevant source parameters, Lambert and his colleagues have carried out an extremely detailed and lengthy study of the surface wave radiation from two explosions and one earthquake. Their particular aims were, first, to determine from surface wave spectra the differences between the long-period radiation from earthquakes and explosions; second, to determine the spatial radiation patterns from the two types and the differences between them; and, third, to describe the individual sources quantitatively. In a comparative exercise of this kind, there is clearly some advantage in studying at least one earthquake-explosion pair with a common point of origin, common recording stations and, if possible, similar radiated energy. In this way, irrelevant phenomena and complicated corrections can be largely avoided.

The events chosen to conform most closely with these conditions were the nuclear explosion SHOAL ($m_b=4.9$) of 1963 and the Fallon earthquake ($m_b=4.4$) of 1962, both of which occurred within 60 km of each other in Nevada and both of which were recorded by the Long Range Seismic Measurement (LRS) network. For further comparison between explosions themselves, the BILBY explosion ($m_b=5.8$) of 1963 was also investigated. Not only was this event of higher magnitude than SHOAL, it was detonated in tuff (by contrast with granite for SHOAL), thus enabling Lambert *et al.* to investigate variations with yield and the nature of the source medium.

The important conclusions are, first, that at long periods the surface wave radiation field from the Fallon earthquake is a quadrupole field corresponding to a quadrupole or double-couple source. At higher frequencies, however, the radiation pattern contains asymmetries indicative of multiple contributions of higher order, probably produced by the propagation of ruptures. Both these results agree quantitatively with theoretical predictions. The surface wave radiation from the explosions, on the other hand, is best explained by sources comprising a superimposed monopole and quadrupole. In the frequency range comparable to that over which the earthquake signal was recorded, there was no suggestion of multipole components of higher order. What Lambert and his colleagues regard as their principal conclusion is that the anomalous double-couple radiation from the explosions is probably not a consequence of earthquake triggering but rather of the relaxation of stress around the shatter zone generated by the shock.

The detailed comparison between the SHOAL explosion and the Fallon

earthquake shows, first, that the ratio of the Love wave amplitudes (earthquake/explosion) decreases with increasing frequency. This implies a larger source for the earthquake. The second result is that, whereas for the explosions the normalized spectral ratio of Love wave amplitude to Rayleigh wave amplitude was close to one and almost independent of frequency, the same ratio for the earthquake was two to three times greater and decreased with increasing frequency. In other words, the earthquake generated Love waves far more efficiently than did the explosions and in a manner dependent on frequency. Finally, Lambert *et al.* thus conclude that these differences might be useful as discriminators between explosions and earthquakes, at least for events with m_b greater than 4.4. On the other hand, as they are careful to point out, there is no guarantee, on the basis of this analysis alone, that all earthquakes behave similarly.

Progress in Gamma-ray Astronomy

Two contrasting contributions in next Monday's *Nature Physical Science* (January 22) emphasize the rapid development of gamma-ray astronomy in recent months. In one, Stecker reviews the present status of theory and observation concerning the diffuse cosmic gamma-ray background; in the other, Guthrie and Tadamaru put forward a model to explain the presence of a low energy (473 ± 30 keV) line in the spectrum of gamma rays from the galactic centre.

The observations of Guthrie and Tadamaru stem from a balloon flight (Johnson *et al.*, *Astrophys. J. Lett.*, **172**, L1; 1972); those of Stecker are chiefly from rocket and satellite experiments, including those on OSO-3 (Kraushaar *et al.*, *Astrophys. J.*, **177**, 341; 1972) and unpublished data obtained during the flight of Apollo 15. The wide range of observational facilities now available is, of course, the underlying reason for the explosion of interest in gamma-ray astronomy, just as in X-ray astronomy.

In the case of the galactic nucleus, Guthrie and Tadamaru suggest that a cluster of neutron stars may be generating the observed line through a mechanism in which electron-positron pairs are first produced in the rapidly rotating intense magnetic fields associated with a neutron star and then annihilated at the star's surface. Gravitational red-shifting of the resulting radiation could then account for the difference between the observed line energy and the theoretical energy for radiation produced in this way (511 keV). Sturrock's studies of pulsars have shown that sufficient electron-positron pairs might be produced for the mechanism to be effective (*Nature*, **227**, 465; 1970), but

RELATIVISTIC ASTROPHYSICS

State of the Art

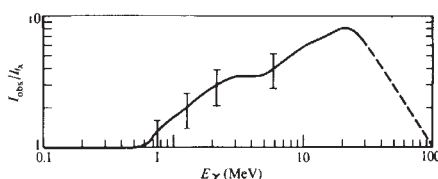
from a Correspondent

THE Texas Symposia on relativistic astrophysics have become biennial reports on the state of the frontiers of astrophysics. During the most recent, held between December 18 and 22 in New York, some fifty speakers summarized the situation for an audience of more than a thousand astrophysicists and physicists.

The first day was devoted to quasars and radio galaxies. Professor H. Arp (Hale Observatories) opened the session by summarizing the case for a non-cosmological origin of redshifts, most of the evidence being based on detailed studies of the unusual members of the extragalactic population. This was followed by several talks given by what could be called anti-Arp forces, who prefer to place emphasis on the normal characteristics of most of this

other suggestions, including the possibility of a nuclear de-excitation line such as that from ${}^7\text{Li}$ (which is stable and has a line energy of 477 keV), cannot be ruled out in the present state of the art.

Stecker's article is concerned with the cosmological implications of the diffuse gamma rays, rather than with events in our own Galaxy. But again the question of matter-antimatter interactions arises. With the new gamma-ray observations, there is an essentially complete spectrum of the background radiation from 10^{-3} to 100 MeV. Above 1 MeV, the gamma-ray spectrum no longer follows the power law typical of X-ray energies, and the ratio of observed flux to extrapolated flux reaches a maximum of 10 near 20 MeV (see diagram). One way in which additional gamma rays might be produced between 1 and 100 MeV is by matter-antimatter annihilation, with a peak at around 70 MeV Doppler shifted to lower frequencies by expansion of the universe. Again, there are many other possible interpretations of the data; but this must be one of the most intriguing possibilities, suggesting the presence of large amounts of antimatter in the universe which is able to interact with the matter there.



Ratio of observed gamma-ray flux to flux predicted by extrapolation from X-ray energies.

population, and conclude that some, and probably all, redshifts are cosmological. Dr L. Woltjer (Columbia University) aptly summarized the attitude of many astrophysicists towards Arp's evidence by saying "it is difficult to believe, but I am still worrying about it".

An independent thread running through the first day was the problem of possible super-relativistic velocities in the radio sources 3C 279 and 3C 120. Dr K. Kellermann (National Radio Astronomy Observatory) pointed out that there are many ways of interpreting the data which do not require "super-c" velocities, particularly using models with components in the source blinking on and off; this interpretation was strongly emphasized by Dr W. Dent (University of Massachusetts). Dr I. Shapiro (Massachusetts Institute of Technology) said that, although radio experiments testing general relativity have not yet distinguished between the Einstein theory and the Brans-Dicke theory, radio observations may yet resolve the question.

The second day was devoted to the experimental and theoretical aspects of gravitational radiation. Dr J. Tyson (Bell Laboratories) did not provide supporting evidence for Weber, but emphasized that Weber's work has resulted in so much growth in the technology for detecting gravitational radiation that definitive experiments, whether negative or positive, may soon settle the problem. A "hot" discussion of the subject erupted in the middle of the scheduled programme a couple of days later when Professor J. Weber (University of Maryland) stated an impromptu "theorem" to the effect that there are an infinite number of incorrect ways of conducting experiments for detecting gravitational waves. He suggested that other workers should more closely follow his methods which have been "proven" to produce results. Tyson and others responded rather spiritedly, and on the whole negatively, to Weber's suggestions.

The esoteric theoretical aspects of gravitational radiation were discussed by Dr R. Geroch (University of Chicago), Professor R. Penrose (University of London), and Professor C. Misner (University of Maryland). Their chief topics were, first, the question of whether considerable energy could be extracted from interactions with singularities (black holes), and, second, whether such singularities would be "naked" or "clothed" (that is, special circumstances could allow radiation to escape from inside a singularity).

On the third day Professor F. Pacini (Laboratorio Nazionale di Frascati) and Professor F. Drake (National Astronomy and Ionospheric Center) reported on the state of the theory and observa-

tions of pulsars. Although some clarifications have occurred and some observational dogmas discredited, the advance of the subject has not been as great as was once hoped.

The striking impact of X-ray astronomy on relativistic astrophysics was evident in the reports of Dr R. Giacconi and Dr H. Tananbaum (American Science and Engineering). The most exciting subject of the symposium concerned the unusual X-ray, optical, and radio events occurring in close binary stars. Tananbaum's discussion of the X-ray eclipsing system Her X-1, and the report by Professor N. Bahcall (Princeton University) on its probable optical counterpart, HZ Herculis, revealed exceptionally interesting data on a binary system which eclipses in both the optical and the X-ray regions of the spectrum, has both short and long term on-off cycles, shows effects of X-rays heating a probable A star to make it appear much hotter on one side, produces pulses by containing a compact object with a "hot spot" rotating every 1.24 s, and shows a host of other interesting details.

The theoretical aspects of X-ray double stars were discussed by Dr K. Prendergast (Columbia University) and Dr D. Pines (University of Illinois). Professor B. Paczynski (Warsaw Institute of Astronomy) summarized what is known about the evolution of close binaries and gave a warning about well known traps awaiting people who interpret data on close binaries too simply. Dr R. Hjellming (National Radio Astronomy Observatory) discussed the radio phenomena associated with both close binary stars and X-ray stars, emphasizing that unusual high energy phenomena must be associated with both types of objects. The workshop on black holes found several supporters for the "almost invisible" component of Cyg X-1 as the currently known object likely to be a black hole.

Details of the X-ray close binaries (Cen X-3, Her X-1, and Cyg X-1) were discussed by Professor R. Ruffini (Princeton University); in particular he considered whether the compact objects that must be present are black holes or "ordinary" neutron stars. The sense of the discussions was that, because of effects by which a star of lower mass could mimic one of higher mass, it was probably not yet safe to declare that Cyg X-1 contains a black hole. The case for this is, however, improving, and in the near future one may be able to say a black hole has been "observed".

The last day of the symposium consisted of talks by Dr R. Omnes (High Energy Laboratory at Orsay) and Dr S. Frautschi (CalTech) about physics at the origin of time, and several talks on miscellaneous subjects, of which the first reports of X-ray observations with

Copernicus presented by Dr P. Sanford (Mullard Space Science Laboratory) and the report by Professor A. Penzias (Bell Laboratory) on the first detection of deuterium in interstellar space were perhaps the most interesting. Penzias reported that deuterium was found in the Orion nebula through detection of radio lines from the molecule DCN; the abundance of deuterium is roughly an order of magnitude greater than expected.

MARINER 1977

Experiments Chosen

EXPERIMENTS planned for the two Mariner missions to Jupiter and Saturn, to be launched in 1977, have been divided into eleven groups, NASA announced recently. The two craft will be identical, although they may follow slightly different trajectories, and will use the gravity field of Jupiter to accelerate them past Saturn; this mission is the remnant of a more ambitious "Grand Tour" plan for one or more spacecraft each to visit all the outer planets by repeated use of this gravitational slingshot effect. That has now been abandoned, even though a suitable alignment of the outer planets occurs only once every 179 yr.

The two missions now proposed will carry instruments for:

- Optical imaging of Jupiter, Saturn, their moons and the rings of Saturn.
- Radiofrequency studies of Saturn's rings, the atmospheres of both planets and of interplanetary space.
- Infrared studies of the planets; this will assist in determinations of atmospheric composition.
- Ultraviolet spectroscopy.
- Magnetometer investigation of the interplanetary magnetic fields as well as those of the two planets.
- A plasma experiment to study the interaction of the solar wind with the planets.
- Analysis of low energy cosmic rays and particles in the radiation belts of the planets.
- Investigation of high energy (interstellar) cosmic rays.
- An experiment to determine velocity and range of interplanetary and interstellar particulate matter.
- Photopolarimetry of the atmospheres of the planets and investigation of reflective properties of their moons and Saturn's rings.
- Studies of the non-thermal radio emission of the planets, their plasma resonances and magnetospheres.

The flight will take four years, covering 807×10^6 mile, and will be planned in detail in the light of the preliminary investigation by Pioneer 10 of deep space and the region near Jupiter.

Do Honey Bees have a Language?

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Von Frisch and later adherents of the theory that honey bees communicate by means of an elaborate dance are challenged by controlled experiments which show that their data can be explained in terms of olfactory cues.

WHenever new data and interpretations are presented which cannot be reconciled with established beliefs, it is proper and desirable that supporters of the traditional view should examine these data and interpretations with great care, and offer such objections as can be generated. In 1969, on the basis of studies of the recruitment of honey bee foragers to food sources, we compared the predictive powers of the classical "language" hypothesis of von Frisch¹ with those of a simple olfaction model for forager recruitment and concluded that olfaction provides the best interpretation of our data².

Investigators who believe that honey bees have a language have since challenged our conclusion³⁻⁵, and two other recent articles⁶⁻⁷, while not including a discussion of our results, cannot be reconciled with our interpretations. Our purpose here is to examine the principal objections to our interpretations and present additional data.

Procedure and Results

From earlier experiments, we knew that scent fed into a hive on one day influences forager recruitment on the next. We established a hive and a line of three experimental sites, each 200 m from it, in a dry field. The sites were approximately 150 m apart and the prevailing light breeze never blew towards or from the hive. Instead, the wind moved from site 1 to site 3, with site 2 in the middle.

During experimental periods of three hours, we fed scented sucrose to ten marked regular foragers at each of sites 1 and 3. Recruited bees were captured and killed. On a following day we fed unscented sucrose at sites 1 and 3 and planted scented sucrose at site 2, which had not been visited by bees from our hive. In this situation, the language hypothesis predicts that recruited foragers would be captured at sites 1 and 3, both visited by successful foragers from the hive. If, however, recruited foragers locate food sources by olfaction, they should arrive at site 2 which was not visited by any bees from the hive.

In four repetitions of this experiment (on days 2, 7, 12 and 16) we captured a total of twenty-five new recruits at site 1, 224 at site 2, and eight at site 3. Thus, newly recruited foragers had located a site in the field not previously visited by them or their hivemates, and had failed to arrive at locations about which they should have been well informed (according to the language hypothesis).

When foragers are experienced at a food site, they are again recruited to it by odour cues rather than by language^{8,9} and, when odour cues are rigorously excluded, recruitment of new foragers fails^{2,10}. For example, on control days of our

experiments reported in 1969 when no scented sucrose was made available (days 4, 9 and 14), arrival of recruits at our feeding stations was drastically curtailed². As a further control, we allowed ten bees to fly to each of four unscented feeders for 3 h (July 25, 1968) for a total of 1,374 round trips. During this period only five recruits from a hive of 60,000 bees found the feeders².

Thus, both experienced and inexperienced foragers seem to require olfactory cues to locate a food source in the field. In the absence of compelling evidence to the contrary, it is conservative to suggest that olfaction alone is sufficient to account for recruitment of honey bees to a food source.

Objections to Our Model

In spite of his polemic, Dawkins³ does offer one substantive objection by arguing that bees are easily distracted from their "intended" (linguistically communicated) goals. In this view, the bees which arrive at site 2 have been distracted from sites 1 and 3, a possibility which deserves serious consideration. But in our experiment, site 2 was downwind from 1 and upwind from 3. Is it likely that recruited foragers could be simultaneously distracted upwind and downwind from their intended goals, or that any animal can be attracted to a downwind odour source? In any case, when all three sites were provided with unscented sucrose (days 9 and 14), recruits failed to arrive at sites about which they were supposedly linguistically informed². We conclude that Dawkins's "distraction" model is not supported by the data.

Esch and Bastian⁶ reported interesting new data but did not relate their findings to the literature on forager recruitment. In each of their experiments, a group of bees was trained to feed at a scent-marked site near the hive, and ten of the foragers were marked and confined temporarily in a cage. The feeder was next moved to a new location 200 m from the hive and the number of regular foragers reduced to one marked bee. The ten marked former foragers were then released and observed, to discover whether they would be re-recruited to the scent-marked feeder at the new location, as they should be on the hypothesis of linguistic communication by dance attendance.

Fourteen of seventy experimental bees did attend dances and subsequently found the new feeder location. Nineteen additional marked bees attended dances and flew from the hive without arriving at the feeder. The remaining thirty-seven marked experimental bees had no contact with the dancer and did not fly to the new location of the feeder.

Ten of the fourteen successfully re-recruited bees required between one and nine exploratory flights with intermediate contacts with the dancer. Only four succeeded on the first flight, two of them within 1 min of flight time. Many of the nineteen unsuccessful dance attenders also made several flights from the hive; they attended between five and thirty-one dances (mean, 17.5) and made between one and nine exploratory flights (mean, 3.4), without finding the feeder.

For the fourteen successfully re-recruited bees the mean time in flight between first attending the dancer and locating the feeder was 8.5 min, compared with less than 0.5 min for

experienced foragers¹¹. Esch and Bastian did not report the durations of flights by unsuccessful dance attenders.

In spite of these negative results, Esch and Bastian consider that their data support the language hypothesis. Because four bees found the feeder on their first flights, it is inferred that, "since they could not have searched the entire experimental area in such a short time . . .", successful re-recruits must have had prior knowledge (obtained from the dance) of the feeder location.

By focusing attention on the performance of successful bees Esch and Bastian have failed to recognize that most of their data actually contradict the predictions of the language hypothesis. The population of experimental animals must be viewed as a whole. In reality, there was a total of 100 exploratory flights by marked bees after attending the dancer. If recruited bees simply flew out equally in all directions from the hive, like spokes in a wheel, 2% would be headed within 7° of the feeder location, a result which is consistent with the observations of Esch and Bastian.

More generally, the data obtained by Esch and Bastian do not bear out the claim by von Frisch¹²: ". . . the tail-wagging dance makes known the distance and the compass direction to the goal. . . . This description of the location enables the newcomers to fly rapidly and with certainty to the indicated flowers, even when these are kilometres away . . . an accomplishment on the part of the bees that is without parallel elsewhere in the entire animal kingdom". Nor do they eliminate the alternative possibility, that re-recruited foragers were flying about, using wind and odour, while seeking the scent to which they had been trained.

The article by Gould, Henerey and MacLeod⁷ contains many data not available earlier, but some of their data do not fall in line with what one might expect from the dance language hypothesis. As in the study by Esch and Bastian, their experiments were ". . . designed to examine the behaviour of individual recruits as each attended a dance and subsequently arrived at a feeding station". In their series 1 experiment, marked bees were trained to two feeding stations 120 m distant from and in opposite directions from the hive, and many workers in the hive were given individual marks. During experiments, of 277 potential recruits observed to attend dances, 240 failed to arrive at any station, and only thirty-seven were subsequently captured at the feeding stations. Of these, a third arrived at a station in a direction opposite to and 240 m distant from that "indicated" in the dance manoeuvre. This result contradicts the prediction of the language hypothesis that all thirty-seven of the successful recruits should have arrived at the "correct" station.

These investigators also found that successful recruits spend a considerable amount of time in flight before reaching the food source. The direct line flight time between hive and a feeding station located at 120 m is less than 25 s¹¹, and recruited bees generally fly from the hive within a minute and a half after leaving a dancing bee (50% leave within 30 s)¹³. The data in Table 4 of Gould *et al.*⁷ reveal that the twenty-five bees which did arrive at the "correct" station flew an average of more than thirty times longer than would be necessary to "fly rapidly and with certainty" to the food source. The twelve bees which ended up at the station in the opposite direction averaged only thirty-six times as long as necessary for a direct flight.

A greater number of marked bees arrived at the station regularly visited by the foragers which recruited them than to the one 240 m from it, and some successful bees found the food source quickly (two in less than a minute), so Gould *et al.* concluded that quantitative information was communicated by the dance. The authors thereby focus attention on successful performers as did Esch and Bastian.

In another experiment, these authors fed sucrose of high molarity at one station and very low molarity at the other.

Most of the dancing in the hive was by foragers regularly visiting the high molarity food source; and during the experiments most of the recruits captured were at that location. This correlation is accepted by the authors as evidence of linguistic communication.

As Gould *et al.* carefully pointed out, the validity of their interpretations for both of the above experiments depends upon the assumption that perfect odour symmetry existed between the two stations. In experiments of this type even small asymmetries of location odour do influence recruitment¹³. Gould *et al.* located their stations in aromatic vegetation. They got uniformly high recruitment whether or not they added scent to the food, yet there is considerable evidence that recruitment is minimal to an unscented location^{2,10}. The high recruitment they obtained indicates that the feeding locations had odours detectable by bees. The unanswerable question is: Did asymmetries of these odours exist between two feeding locations 240 m apart?

Mautz⁵ also was concerned with the behaviour of individually marked workers which came in contact with a forager dancing in the hive. In his experiments, 32% of marked workers that attended dances found the feeder, and the amount of time the potential recruits attended the dancer was positively correlated with success at finding the food. In addition, the average flight time for successful recruits was more than ten times that expected of experienced foragers and the mean flight time of bees which flew out but failed to find the feeder was twice that of the successful recruits. Mautz noted that one bee found a goal at 400 m after following only five waggle cycles of the dance.

As with Gould *et al.*, and with Esch and Bastian, Mautz assumed that his feeder locations were not recognizable by bees as olfactorily distinctive and that only linguistic information could be communicated more effectively by increased duration of contact with the dancer. He does not discuss the possibility that a recruit which spends more time attending a dancing bee might, by so doing, gain a more accurate impression of the odour characteristics of a specific location. Again we see a supporter of the language hypothesis focusing attention on the successful recruits.

Of the several papers we review, Lindauer's⁴ was the most deliberate and direct effort to obtain data contradictory to our findings. The first type of experiment undertaken by Lindauer resembled those of Gould *et al.*⁷ in design, and yielded similar data. Forager bees were trained from a hive to two feeding sites, and were then fed high molarity sugar at one and low molarity sugar at the other. Virtually all dancing was by bees visiting the high molarity sugar site, and most successful recruits landed there. Lindauer assumed, as did Gould *et al.* and Mautz, a perfect symmetry (to bees) of environmental odours at his locations. From this he inferred that, "if the recruits followed only the odour signals given by the dancers . . . they should have appeared in equal numbers at (both) sites". In accordance with his assumption Lindauer attributed the asymmetry in recruit arrivals to linguistic communication. Thus, the untested assumption of station symmetry (to bees) is central to the reasoning of Lindauer as well as to the above authors.

In his second experiment, Lindauer put out three stations in a geometry similar to that used in our experiments² and trained bees to stations 1 and 3 but not to 2. Then, with scented sucrose at all three stations (a variation from our design), he collected recruits. Although the stations were deliberately asymmetrical (no foragers at station 2), approximately one-fourth of all captured recruits arrived at station 2. The language hypothesis predicts that the recruits should have travelled only to stations 1 and 3.

In order to reconcile these data with the language hypothesis Lindauer generated *ad hoc* the auxiliary hypothesis that potential recruits alternate their attentions between dancers visiting sites 1 and 3, and subsequently "integrate the directions communicated by both groups of foragers".

Although the "integration" model satisfies Lindauer as an explanation of his data, it could not possibly predict the distribution of recruits in our earlier experiments^{14,15} or, indeed, in the recent experiments of Gould *et al.*⁷

Lindauer got good recruitment at his stations 1 and 3 when presenting unscented food at those locations. Interpreted in the light of our data^{2,10} these results suggest that Lindauer's feeding sites were in some measure distinctive to bees. This may explain the asymmetry of recruitment he observed when foragers to station 1 were killed and bees visited only a single station. Unfortunately, the precise location and ecology of the sites for these experiments are not given in his paper (except that it was near Frankfurt), nor does he provide dates, wind speed and directions, number and sequence of observations contributing to the total data and other relevant information. These omissions make any interpretation difficult and speculative. Another portion of Lindauer's paper offers some philosophical and rhetorical objections to our paper, but we will consider these later.

Thus all the authors we have discussed⁴⁻⁷ explicitly or implicitly assumed that the feeding stations they established in the field were in no way distinctive to bees and that the environment can be made symmetrical with respect to odours and other factors. They also assumed that, in order to find the food, a recruit must have prior quantitative information about its location. These assumptions lead the authors to interpret arrival of new workers at sites visited by dancing foragers as definitive evidence of linguistic communication. As pointed out earlier they have focused their attention on successful recruits. Experiments based on these assumptions invariably lead to affirmation of the consequence of the hypothesis (if bees have a language, recruits will reach the food. Some recruits find the food. Therefore, bees have a language). This reasoning is deductively invalid¹⁹.

The experimental designs of these investigators suffer yet another serious disadvantage. It is not logically possible experimentally to establish the validity of the station symmetry assumption (failure to display asymmetry does not prove that none exists). It is quite practical, however, to seek positive evidence of station asymmetry in experiments of this design. One way would be to remove all directional information from dances performed by foragers regularly visiting food sites. If, in spite of this, recruits preferentially arrive at the feeder regularly visited by the foragers which recruited them, it could be concluded that they had done so without use of prior directional information, and that honey bees can exploit subtle environment asymmetries in the recruitment of new foragers to food sources.

We have incorporated the use of "directionless" dances in the design of the experiments reported below.

Conditions and Observations

In the summer of 1970 we did experiments using a single frame observation hive and two feeding stations located 150 m from it in approximately opposite directions (west and east of the hive, but slightly north of it). The hive and stations were located at the University of California, Santa Barbara. Both stations were located in dry fields, devoid of green vegetation. The West Station was nearer the sea and upwind; the East Station somewhat closer to the University buildings. Otherwise, the sites appeared to be symmetrical.

Ten marked foragers were trained to feed at each station; all successful recruits were either killed or marked as replacements for regular foragers which were then killed. Observations were made at the hive and stations during 3-h periods (0830-1130), August 21 to September 4, 1970, and food was provided only during those periods. A westerly breeze prevailed during this period (up to 5 mile h⁻¹ on most days) and the usual temperature was 65°-75° F.

A principal objective of the experiments was to examine relationships among food molarity, forager behaviour, and

recruitment. Therefore, scent added to the food was held constant. All solutions used were scented with lavender oil (32 μ l. l.⁻¹). Sucrose solutions of 0.8 M, 1.3 M, 1.8 M and 2.3 M were used. On any given day a solution of lower molarity was offered at one station and one of higher molarity at the other. Rotation of molarities between the stations controlled for possible inherent differences in location attractiveness, and on each day the stations acted as controls against each other. The experiments were run blind: the observers were not informed of molarities of sucrose provided at the feeding stations on any given day.

The hive had glass sides and was equipped with a clear plastic atrial chamber inside a diffusely lighted observation room. Hive conditions (well populated, and with little natural forage) ensured that our marked foragers danced in the atrial chamber, rather than on the comb. These dances were on a horizontal surface, and contained no discernible directional information. The straight (waggle) portions of the dance were apparently randomly oriented and highly variable within each dance episode.

Altogether, a total of 1,793 dances by our marked bees and 136 dances by unmarked bees foraging on natural sources were observed during eleven days (33 h) of experimentation. Thus, during our study periods, 93% of all dancing in the hives was by our marked foragers.

Although we were largely successful in eliminating oriented dances from our hive, occasionally a marked forager did enter and perform a dance episode on the vertical surface of the comb. These occurrences will be of great interest to proponents of the language hypothesis, so we will present data on oriented dances before proceeding to other aspects of the study.

There was a total of only 50 apparently oriented dances recorded during 33 h of experimentation; 1.5 such dances h⁻¹, on the average, or approximately 2.8% of all dance episodes recorded for our marked foragers. These occurred equally (24 to 26) for the two stations and did not correlate with recruitment (Table 1).

These oriented dances were not distributed evenly throughout the experimental period, but occurred sporadically. For example, 40% of them occurred on one day, August 30, 1970. Occurrence of a block of oriented dances on a given day did not appear to influence recruitment. A comparison of data for August 30, 1970 (which had the greatest number of on-comb dances during the eleven days of experimentation) with the following (more typical) day is illustrative, and is presented as Table 2.

Table 1 Dances by Unmarked Bees, Oriented Dances, Disoriented Dances, and Recruitment

	First hour	Second hour	Third hour	Total
Dances by unmarked bees	77	38	21	136
Oriented dances by marked bees	23	9	18	50
Disoriented dances by marked bees	337	675	731	1,743
Successful recruits	64	115	143	322

Data are partitioned according to time of occurrence in our experiments. Total data for 33 h of observation are presented.

Table 2 Comparison of Visitation by Marked Foragers, Oriented and Disoriented Dances by Marked Foragers, and Successful Recruitment of New Bees for August 30, 1970 (with many Oriented Dances) and August 31, 1970 (with few)

	Trips by marked foragers		Oriented dances		Disoriented dances		Successful recruits	
	1.3 M	1.8 M	1.3 M	1.8 M	1.3 M	1.8 M	1.3 M	1.8 M
August 30, 1970	319	267	9	19	76	110	20	65
August 31, 1970	304	312	1	2	59	143	19	67
Total	623	579	10	21	135	253	39	132

Table 3 Mean Number of Trips by Marked Foragers; of Disoriented Dances by those Foragers; and of Successful Recruits per 3 h Observation Period for 0.8 M, 1.3 M, 1.8 M and 2.3 M Lavender Scented Sucrose Solutions

Molarity	Forager trips	Disoriented dances	Successful recruits
0.8	308	26	5
1.3	320	86	19
1.8	293	117	42
2.3	231	107	17

Not less than four or more than seven observation periods at each molarity.

Table 1 also shows that dancing by unmarked bees foraging on natural food sources declined as the experiment progressed. This is consistent with our findings that insertion of a new food source in the hive-environment system may change the behaviour of bees regularly foraging on an established one¹⁰. Presumably this alteration is mediated through a change in the available recruit pool.

From our data on oriented dances (Tables 1 and 2) it is not possible to infer that these dances are responsible for the observed recruitment of new foragers to those feeders we had established in the field. There is, however, a positive correlation between the number of disoriented dances (or total dances) and recruitment of new foragers (Table 3).

The experimental design is not rigorous enough to define the asymmetry in environmental cues used by recruits while searching for and locating a particular feeder in the field, but quantitative directional information does not appear to be a part of that system.

While doing this set of experiments, we also gathered information on Nasanov gland exposures at the food source by bees foraging on various molarities of sucrose. Our previous studies indicate that Nasanov gland exposure by regular foragers fails to attract undisturbed bees but apparently does provide a point of reference for disoriented members of the colony. Gland exposure is apparently a function of the interest potential recruits may have in a food source, rather than food quality *per se*^{2,10}. Nasanov gland exposure by foragers visiting established food sources in the field was depressed at the lowest molarity we used (Table 4), a fact which indicates that few recruits were in the field searching for that source.

Further Considerations

We have done several experiments which were designed to determine whether honey bees use linguistic communication under defined conditions^{2,8,9,14-16}. Results indicated that the language hypothesis has little heuristic value and fails as a predictive tool. The results further suggest that if one wished bees to pollinate a particular crop, it should be useful to regulate the odour carried into the hive by foragers, rather than the angle of dance on the comb.

More generally, we feel that honey bee foraging ecology is regulated by a complex system of hive, environmental and behavioural variables. With sufficient knowledge, it should be possible to manipulate the system and predict the consequences for the foraging ecology of a honey bee colony. We then might say that we understand colony function in terms which are neither teleological nor anthropomorphic; and we would be in a position to attack pragmatically

Table 4 Relationships Among Molarity of Sucrose Solutions Provided, Forager Visitations, and Nasanov Gland Exposures by Marked Foragers

Molarity	Forager trips	Nasanov gland exposures
0.8	308	24
1.3	320	77
1.8	293	85
2.3	231	71

Means for 3 h observation periods are given ($7 \geq \text{periods/molarity} \geq 4$).

problems of a comparative or practical nature. Accordingly, we have examined interrelationships among Nasanov gland exposure, visits of marked foragers to a food site, dancing in the hive, and recruitment of new foragers, and correlated these with the amount of odour in sucrose solutions provided in the field. In these experiments we have had some success in manipulating the system¹⁰, as have Waller¹⁷ and Friesen¹⁸.

Waller showed that association of odours with food in the field, but not odours alone, could increase bee populations in experimental plots¹⁷. Friesen examined interrelationships among wind speed and direction, forager flight paths, odour levels and locations, forager visitation and recruitment of new foragers to sites in the field. He concluded that, after recruits leave the hive, their success and distribution depend upon an interaction of field variables affecting the distribution of odours¹⁸. Thus, evidence from several sources indicates that odour is of great importance in the system of variables which regulate honey bee foraging ecology.

Our comparison of the predictive values of the olfaction and language hypotheses of honey bee forager recruitment was an attempt to determine just how important odour is in this system². To our surprise, our results and interpretations generated a controversy which deeply polarizes interested biologists. Along this line we must stress that the controversy does not emerge from a difference of opinion about the acceptability of various parts of a body of evidence.

The ingenious step and fan experiments of von Frisch have been repeated many times, and our own published data clearly show that when his procedures are closely followed, without insertion of additional controls, distributions of successful recruits may be expected to resemble those obtained by him^{14,15}. Similarly, when Lindauer used an experimental geometry modelled after ours, he obtained data consistent with ours⁴; and the data presented in Table 3 of this paper resemble those obtained by Gould *et al.* and by Lindauer when they used similar experimental designs^{4,7}. Neither the repeatability of experiments on honey bee behaviour nor the care and accuracy with which workers in this field gather and report data appears to be in question.

Proponents of the language hypothesis argue that the many repetitions of the von Frisch experiments make it highly probable that his interpretation is correct. One must realize, however, that the amount of additional confirmation affected by each new favourable repetition of an experiment becomes smaller as the number of previous repetitions grows¹⁹. Thus, even under the most favourable conditions, sheer number of repetitions is never sufficient to render a hypothesis immune to challenge. Furthermore, in the case of the bee language hypothesis, the existence of a considerable body of unfavourable evidence greatly diminishes the weight of even a large body of confirming data²⁰.

Any hypothesis which is generated as an explanation of certain observed events will, of course, imply their occurrence. The events to be explained will then be taken as supporting evidence for it. If the hypothesis is valid it also may lead to *a priori* predictions of facts and events in conditions different from those leading to its formulation.

It is exactly here that we have difficulty with the language hypothesis of honey bee orientation. Each time we have inserted previously omitted controls or altered the experimental design to create new test implications, the distribution of successful recruits to feeders located in the field is no longer that predicted by the language hypothesis. Similarly, much of the evidence recently obtained by Esch and Bastian, by Mautz, by Gould *et al.* and by Lindauer is contrary to the predictions of that hypothesis⁴⁻⁷.

The difficulties we had in reconciling our data¹⁴⁻¹⁶ with the language hypothesis led us to propose the alternative olfaction model^{21,22}. The olfaction hypothesis allowed a satisfactory explanation of the distribution of successful recruits in those experiments which incorporated new

controls¹⁴⁻¹⁶ and could encompass the large body of existing data on honey bee forager recruitment. Furthermore, it had the advantage of simplicity.

Thus, in 1967 we had two hypotheses, olfaction and language, each generated *a posteriori* to explain an existing body of evidence. The language hypothesis had been well articulated by von Frisch twenty years earlier¹, but it had been expanded and modified through the years in an attempt to explain newer results. Olfaction, while new in the sense that it challenged the then currently accepted hypothesis and two decades of thought habits, was essentially a return to the position held by von Frisch and others in the earlier years of this century.

Lindauer not only presented new data but challenged our interpretation on philosophical grounds when he invoked Aristotelian (Darwinian) teleology as an argument in favour of language. He argued that in nature "each morphological structure and behavioural act is associated with a special function. On this basis alone, it would seem unlikely that information contained in the waggle dance of a honey bee is not transmitted to her nest mates"⁴. We have answered his challenge in part by discussing the relevant data in terms of philosophy of science which we consider to be more powerful^{19,20}.

An inherent weakness of Lindauer's teleological argument can be illustrated by giving one interesting example from another field of biology. Methyl eugenol is extremely attractive to male oriental fruit flies. In one test in Hawaii 1,300 male *Dacus dorsalis* were attracted a half-mile upwind to a muslin screen that had been treated with it. Methyl eugenol is not produced by the female fruit flies nor does it attract them. It is not a component of the natural food of this fly and probably has no nutritional value. Yet male oriental fruit flies are irresistibly attracted to it and "apparently cannot stop feeding when they have free access to it, and they kill themselves with over indulgence"²³. Their behaviour under these circumstances certainly cannot be construed as adaptive.

Admittedly, this is an extreme example of non-adaptive behaviour, but it does reveal a weakness in the teleology argument. The mere presence of a characteristic behavioural pattern in an animal cannot be construed as purposeful, adaptive or "associated with a special function".

Lindauer⁴ also challenged us on grounds that we have not individually discussed three specific situations investigated earlier by von Frisch and his colleagues. First, at distances quite close to the hive, recruited foragers may be captured at scented dishes other than the one visited by the foragers which recruit them, while at distances of 100 m and beyond "the recruited workers all fly in the direction of the feeding plate". That the latter part of this quoted statement does not hold is well documented by the data of Gould *et al.*⁷ as well as by ours^{2,14,15}, and even by Lindauer's own data⁴ unless one is willing to accept his "integration" model. As for the first part of the statement, when the feeding dishes are close to the hive, we agree with von Frisch and Lindauer; the bees seem to be using odour.

A second situation involves disoriented dances performed by foragers on a horizontal surface. In an experiment using a tilted hive, von Frisch got approximately equal recruitment in four directions while regular foragers were fed in only one direction. Later that day, with the hive put upright again, he got preferential recruitment in the direction of the feeder visited by foragers¹². We prefer a design in which two or more stations visited by bees simultaneously serve as controls against each other, and we set up our experiment accordingly. Our data on the effectiveness of disoriented dances (Table 3) seem to be in disagreement with those of von Frisch. During our 11 days of observation, new recruits preferentially arrived at the sites visited by dancing foragers even though there was no directional component in the dances.

The third situation involves the detour of foragers around an obstacle to a feeder on the other side of it. In one experiment performed by von Frisch, marked foragers were trained to fly around a rocky ridge to a scented feeder. Scent plates were placed on top of the ridge and 50 m laterally to the direct line between hive and feeder. During a 90 min period, three new recruits were captured at the lateral stations, eight on top of the ridge and twenty-three at the feeder. Because the new recruits apparently failed to follow the flight path used by the foragers which recruited them, von Frisch feels that they were linguistically informed of the feeder location¹².

We must argue that the flight paths and duration of searching by the twenty-three successful recruits are not known, and that a failure of bees to use the detour (if indeed they did not use it) does not differentially support either language or olfaction. Since neither hypothesis predicts that recruits will follow the same path as the marked foragers, the experiment is not crucial and the results do not support or refute either hypothesis.

We believe that experimentation does not "prove" or "disprove" hypotheses, but rather affects their credibility. If the credibility of a hypothesis at any given time is determined by the total body of relevant information available, then the language hypothesis of forager recruitment was very credible in 1946 when von Frisch proposed it. It is less so now, for the body of relevant information is quite different and includes much unfavourable evidence. In fact, it is so much less credible that we no longer can believe that honey bees communicate linguistically.

Do honey bees have a language? That is a question which may never be answered with certainty. It may be more useful to examine assumptions critically, state hypotheses and their consequences with precision, review the evidence objectively, and ask: can we now believe that honey bees have a language? Thus, it appears that the honey bee forager recruitment controversy is not about the nature of evidence but rather about the nature of hypotheses²⁵. It is not what investigators observe (the data) but what they believe (infer) that is at the heart of the controversy.

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Biostratigraphic and Geological Significance of Miocene-Pliocene Volcanism in Fiji

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Dating of Fijian volcanic rocks enables an estimate of $4.9 \pm 0.4 \times 10^6$ yr to be made for the age of the Miocene-Pliocene boundary. The change in composition of the volcanism in Fiji between about 5 and 6×10^6 yr ago may result from migration of the site of subduction.

ESTIMATES of the age of the Miocene-Pliocene boundary have ranged widely over the past decade from about 13×10^6 yr to 3×10^6 yr. This large spread arises partly because of problems in finding appropriate rocks to date by isotopic methods, and partly because of difficulties involved in stratigraphic and palaeontological correlations. Definition of planktonic foraminiferal zones¹ has facilitated global correlations. The Miocene-Pliocene boundary defined in Italy lies within zone N.18 and this zone has been recognized in certain stratigraphic sequences in the South Pacific.

We have used the K-Ar method to date some volcanic rocks from Fiji that are related stratigraphically to sediments containing diagnostic Neogene planktonic and benthonic foraminifera. Volcanic rocks in island arcs are inherently difficult to date by the K-Ar method because they are often porphyritic, because they may have formed under modest to high partial gas pressures, and because much of the potassium occurs in a groundmass which often contains considerable glass. The first two factors increase the likelihood of obtaining spuriously old ages due to the presence of extraneous or initial radiogenic Ar. The third factor makes it difficult to establish firm criteria by which to predict whether or not samples containing apparently undevitrified glass will give ages that are correct or only minimum estimates.

The samples used in this study are from collections by Gill² and Tarling³. All samples were examined in thin section before dating; those that showed more than incipient alteration were rejected. Analytical results and other appropriate information are presented in Table 1. Our analytical methods have been described elsewhere⁴.

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Age of Volcanogenic Units for Viti Levu, Fiji

Rocks of the Mendrausuthu Volcanic Group crop out in south-eastern Viti Levu and primary descriptions of them appear in several bulletins of the Fiji Geological Survey Department⁵⁻⁷. This group has been interpreted⁸ as a "volcanogenic unit" composed of undeformed lavas and auto-brecciated agglomerates which grade laterally into conglomerates, greywackes, mudstones, and marls⁹. The geochemistry and genesis of the parental volcanics (the Namosi Andesite) will be discussed in detail elsewhere (J. B. G., in preparation), as have the faunas of the related sedimentary rocks^{5,6,10-15}. The more distal facies include the Veisari Sandstone and the Suva Marl, which contain particularly rich faunas with planktonic foraminifera indicative of zones N.17, N.18, N.19. Basal sediments of several formations within this group contain detritus of plutonic rocks which are thought to be 9 to 11×10^6 yr old¹⁶. From these data the volcanism and volcanoclastic sedimentation are usually assigned to the late Miocene to earliest Pliocene. But the faunal evidence is internally inconsistent and was summarized as such by Ibbotson⁶, who nevertheless favoured a late Miocene age. Ladd¹¹, Whipple¹³ and Ladd (personal communication) maintain that the Suva Marl belongs to the Tf and probably Tf₁₋₂ stage of the East Indies classification. The Tf₁₋₂ letter stage is regarded as middle Miocene, equivalent to zones N.9 to N.12 (refs. 17, 18) and older than 12.5×10^6 yr (refs. 17, 19). Rodda (ref. 20 and personal communication), on the other hand, interprets the foraminifera as indicating either a Tg stage biocoenosis together with reworked fossils from underlying strata, or that the larger foraminifera were contemporaries with planktonic foraminifera of zones N.17 to N.19. Ladd (personal communication) regards the larger foraminifera in question as too well preserved and abundant to have been reworked.

The same internal inconsistency seems to occur within the Futuna Limestone of the nearby Lau Islands, Fiji. This formation has been considered to be lower Tf stage and correlative with the Suva Marl on the basis of these same larger foraminifera and molluscs²⁰⁻²². But the planktonic foraminifera which dominate certain horizons²³ are indicative of zones N.18 and N.19 (particularly *Globorotalia* (G.) *multicamerata* [=G. *menardii* var. *fijiensis*], *Globigerina conglomerata*, and *G. bulloides*). Discordant K-Ar results² suggest that underlying volcanics are 5 to 9×10^6 yr old but

these might be minimum ages only.

We have dated three whole rock samples of Namosi Andesite and hornblende separates from two of these rocks. The ages are all concordant at $5.7 \pm 0.1 \times 10^6$ yr (Table 1), providing strong evidence that this is the age of eruption and crystallization of these rocks. Each sample has phenocrysts of labradorite and hornblende set in a fine-grained, holocrystalline groundmass of plagioclase, hornblende, an opaque and clinopyroxene. Samples 69-874 and 69-875 are clasts from agglomerate near Namosi village, whereas sample 68-66 is from an intrusive stock which forms a satellite volcanic centre (Mau quarry) about 10 km south-south-east of the others. The exact stratigraphic relationship between these samples and the Suva Marl and Veisari Sandstone horizons from which the above-mentioned fossils were collected is not known with any precision. But hornblende-bearing volcanics occur only at the stratigraphic

Wainikaro Creek. The measured K-Ar age of $6.1 \pm 0.5 \times 10^6$ yr (Table 1, 69-870) supports the interpretation of Band⁵ that the stock is cogenetic with the Namosi Andesite, rather than older as suggested by some other data¹⁶.

The Koroimavua Volcanic Group is a second volcanogenic unit^{8,25,26} and crops out in western Viti Levu. Faunal data are much less abundant but Blow¹ assigns tuff sample PEK/F.4 to zone N.18, so that this group is also regarded as having been deposited at or near the Miocene-Pliocene boundary. The group is underlain by strata thought¹⁰ to be no older than upper Miocene. Conformably underlying sample PEK/F.4 are the lava flows and agglomerates previously known as the Sambeto Volcanics. Biotites separated from two specimens collected about 800 m apart gave concordant K-Ar ages of $5.35 \pm 0.1 \times 10^6$ yr (ref. 27). Two biotite separates from a nearby monzonite plug, thought to be an associated volcanic centre, yielded ages of $4.9 \pm 0.1 \times 10^6$ yr.

Table 1 Potassium-Argon Ages on some Whole Rocks and Minerals from Viti Levu, Fiji

Sample No.	Material	Tarling ³ site No.	K (wt.%)	Rad. ⁴⁰ Ar (10 ⁻¹⁰ mol/g)	100 Rad. ⁴⁰ Ar / Total ⁴⁰ Ar	Calculated age (× 10 ⁶ yr ± 2 s.d.)	Magnetic polarity ³	Rock type and locality
A. Namosi Andesite; Mendrausuthu Volcanic Group (no stratigraphic relationships known)								
68-66	Whole rock	4	1.126, 1.137	0.1151	64.7	5.71 ± 0.10	R	Andesite*, Mau quarry, 178° 14' E, 18° 11' S
68-66	Hornblende	4	0.314, 0.315	0.0326	20.9	5.80 ± 0.34	R	
69-874	Whole rock	—	1.349, 1.351	0.1390	18.1	5.77 ± 0.19	—	Dacite*, Wainanggara
69-874	Hornblende	—	0.288, 0.283	0.0291	18.1	5.71 ± 0.32	—	Creek, 178° 7' E, 18° 3' S
69-875	Whole rock	—	1.653, 1.644	0.1632	23.9	5.55 ± 0.18	—	Andesite, same site as 69-874
69-870	Hornblende	—	0.330, 0.328	0.0356	9.7	6.1 ± 0.5	—	Diorite*, Wainikaro Creek stock, 178° 13' E, 18° 2' S
B. Mba Volcanic Group (in stratigraphic sequence)								
69-894	Biotite	—	7.593, 7.587	0.593	62.6	4.38 ± 0.06	—	Banakitite* or "biotite trachyandesite", Mbasala Creek, 177° 52' E, 17° 32' S
72-472	Whole rock	50	1.644, 1.652	0.1146	42.0	3.90 ± 0.10	N	Absarokite, Nandarivatu Rd., alt. ~620 m
72-473	Whole rock	51	1.529, 1.529	0.1149	29.3	4.22 ± 0.10	N	Absarokite, Nandarivatu Rd., alt. ~570 m
72-474	Whole rock	51	1.289, 1.297	0.1045	18.6	4.53 ± 0.19	N	Absarokite, same site as 72-473
68-73	Whole rock	54	3.358, 3.349	0.2640	85.7	4.42 ± 0.06	R	Shoshonite*, Nandarivatu Rd., alt. ~400 m

* Indicates full chemical analysis available in refs. 2, 10 or 43. $\lambda_e = 0.585 \times 10^{-10}$ yr⁻¹; $\lambda_\beta = 4.72 \times 10^{-10}$ yr⁻¹; $^{40}\text{K}/\text{K} = 1.19 \times 10^{-2}$ atom %.

top of the Namosi Andesite formation^{5,20}. R. Todd has identified a siltstone from above or associated with the uppermost volcanics in this formation as belonging to zone N.18 (P. Rodda, personal communication). She also found *Sphaeroidinella dehiscentis* in the Veisari Sandstone, a lateral equivalent of the Namosi Andesite and Suva Marl, within which there is detritus from the satellite volcanic centre represented by sample 68-66 (ref. 5). Thus by studying these three Namosi Andesite samples we believe that we have dated volcanism which occurred during the time represented by a portion of zones N.17 to N.19 and the Tg letter stage, at or near the Miocene-Pliocene boundary. Tarling³ found five samples collected from the same site as sample 68-66 (his site 4) to be reversely magnetized.

We also dated hornblende separated from a coarse grained hornblende gabbro sample collected from a stock in

Tarling³ found three samples (site 45) collected near those dated by McDougall to be normally magnetized.

It is salutary to note that when McDougall²⁷ reported these K-Ar data for the Koroimavua Volcanic Group he questioned the palaeontological dating for the sediments because it was believed at that time that the age of the Miocene-Pliocene boundary was firmly established at 12 to 13 m.y. This age for the boundary was obtained from correlations of isotopically well-dated mammalian faunas of North America into the European stages. Subsequent data from marine sequences have rendered this earlier correlation increasingly questionable.

A third volcanogenic unit, the Mba Volcanic Group, occupies the northern portion of Viti Levu and is the best-studied geologically because of its associated Au-Ag-Te mineralization at the Tavua centre²⁸⁻³⁰. Distal greywackes

contain foraminifera loosely correlated²⁶ with the Tg and Th letter stages and include the planktonic species *Globigerina nepenthes* which became extinct late in zone N.19 (ref. 1). Rocks of the Mba Volcanic Group conformably overlie those of the Koroimavua Volcanic Group where they are locally in contact^{20,26}. The volcanic rocks themselves have been subdivided into five phases³⁰.

Our four whole rock samples from the Mba Volcanic Group were collected by Tarling³ from a section of apparently conformable lava flows separated by pyroclastics which crop out along the road between Waikumbukumbu and Nandarivatu. But Rodda suggests (unpublished) that some of these bodies may be intrusive rather than extrusive. The sites span a vertical distance of about 240 m and all belong to the oldest period of Mba volcanism (phase 1) from the Tavua centre. Their relationship to the distal foram-bearing sediments is not known. They contain phenocrysts of olivine and clinopyroxene with some plagioclase and an opaque. The olivines are partly altered to iddingsite, and minor zeolite occurs along pyroxene boundaries in samples 72-473 and 72-474. The groundmasses are holocrystalline and generally fresh, and include plagioclase, K-feldspar, clinopyroxene, an opaque, and some (<2%) zeolite. These four samples define a sequence of events occurring between 4.4 and 3.9×10^6 yr ago (Table 1). Gale (in ref. 3) reported a $4.3 \pm 0.2 \times 10^6$ yr K-Ar age for one sample from Tarling's site 53, stratigraphically intermediate between our samples 68-73 and 72-474. Rodda *et al.*¹⁶ published K-Ar ages of $\sim 7.5 \times 10^6$ yr (minimum age, whole rock) and $10 \pm 0.5 \times 10^6$ yr (biotite) for two samples from levels 16 and 10, respectively, of the Emperor Gold Mine at the Tavua centre. We also dated biotite separated from what is locally known as "biotite trachyandesite", which represents the youngest phase of Tavua volcanism (Table 1, sample 69-894). Its age of $4.38 \pm 0.06 \times 10^6$ yr is older than those of some of the whole rocks discussed above which by stratigraphic criteria³⁰ should be older. Rodda *et al.* (ref. 16, sample X.27) reported a $4.7 \pm 0.2 \times 10^6$ yr age for biotite collected from the same outcrop. They also reported five other ages between 5.0 and 4.3×10^6 yr for whole rock or separated biotite samples from horizons younger than phase 1 volcanics. The 7 and 10×10^6 yr results clearly are anomalous. The rest of the measured ages lie between 3.9 and 5.0×10^6 yr and are slightly discordant. Nevertheless these data indicate that Tavua volcanism probably was confined to the interval between about 5 and 4×10^6 yr ago.

The Miocene-Pliocene Boundary

The Miocene-Pliocene boundary must be considered within the framework of the classic Neogene stratigraphy of Italy. Selli³¹ discussed the upper Miocene Messinian stage and defined a neostratotype which is overlain by the "trubi" marl of the Zanclean stage, widely accepted as lower Pliocene. Using this definition, Blow (ref. 1, p. 418; also his Fig. 19) regards the Miocene-Pliocene boundary as lying within foraminiferal zone N.18, just below the base of zone N.19. This latter zone is defined by the first evolutionary appearance of *Sphaeroidinella dehiscentis* which occurs in the section chosen by Selli³¹ about 13 m above the base of the 65 m thick "trubi" marl, or about 13 m above the level of the type Miocene-Pliocene boundary.

The Fijian data presented here are among the few radiometric ages available for samples that are closely associated with sedimentary sequences assigned to foraminiferal zones which straddle this boundary. Our Namosi Andesite samples dated at 5.7×10^6 yr are lateral equivalents of the Veisari Sandstone and Suva Marl, samples from which have been placed in zones N.17, N.18, and N.19 by Blow¹ and Parker¹⁵. Significantly, some of the stratigraphically lowest samples contain *Sphaeroidinellopsis subdehiscentis*, the ancestor of *Sphaeroidinella dehiscentis*, which itself appears

high in the sequence. Unfortunately, stratigraphic information with which to relate the various formations of the Mendrausuthu Volcanic Group or the various fossil sites with the Suva Marl and Veisari Sandstone to one another in detail has not been published. Both the Namosi Andesite and the Koroimavua Volcanic Group (which has measured biotite ages of 5.3×10^6 yr) are, however, each overlain by at least one sample which has been assigned to zone N.18. Volcanic detritus from an outlying Namosi Andesite centre also dated at 5.7×10^6 yr is interbedded with N.19 foraminifera. Because the Miocene-Pliocene boundary lies near the top of zone N.18, we conclude from these Fijian data that its age is no older than 5.7×10^6 yr and is equal to or slightly younger than 5.3×10^6 yr.

This requires that the larger foraminifera mentioned earlier are either from considerably older horizons within the Mendrausuthu Volcanic Group, or are reworked, or lived until upper Miocene time, at least in Fiji. The larger foraminifera are found in horizons stratigraphically between samples assigned to N.17 and N.18 (Rodda, unpublished), which excludes the first alternative. There is no morphological evidence for the second possibility, and the third disagrees strongly with the correlations of Clarke and Blow¹⁸, Adams³² and Berggren¹⁷. Thus the larger foraminifera seem the least definitive of the available biostratigraphic indices and their occurrence in association with 9 to 11×10^6 yr old plutonic detritus in the Mendrausuthu Volcanic Group is compatible with all the correlations offered by Berggren¹⁷, rather than favouring his third alternative.

There are few other radiometric age measurements which are directly relevant to the Miocene-Pliocene boundary as defined in marine sequences. Micras from granitic rocks on Elba Island yielded K-Ar and Rb-Sr ages of 6 to 7.55×10^6 yr (ref. 33). Sediments exposed nearby on the mainland of Italy contain gravels, fragments in which are thought to have been derived from Elba. The stratigraphic level at which the gravels first occur is earliest Pliocene (Tabianian) (ref. 34), so that the Miocene-Pliocene boundary must be younger than the isotopic age of the granites. Tongiorgi and Tongiorgi³⁴ suggested that the boundary lies between 6 and 7×10^6 yr, an interpretation tentatively accepted by Funnell³⁵. Nevertheless these data are consistent with a younger age for the boundary as postulated above. In Morocco, Charlot *et al.*^{36,37} found K-Ar ages of 8 to 6.6×10^6 yr for micras from tuffs interbedded with sediments correlated with the Messinian stage, giving support to the view expressed by Tongiorgi and Tongiorgi. A number of K-Ar dates are available for younger Neogene samples from marine sequences in western North America but there are difficulties interpreting these results because of major internal inconsistencies discussed principally by Bandy and Ingle³⁸ and Bandy *et al.*³⁹.

In studies of microfossils from deep-sea cores, magnetic stratigraphy and the standard polarity time-scale can be used to provide physical age control. Hays *et al.*⁴⁰ record a major faunal change at about 3.0×10^6 yr ago within the Mammoth reversed event, at which level *Sphaeroidinella dehiscentis* increases greatly in abundance. Bandy *et al.*³⁹ refer to this as the "*Sphaeroidinella dehiscentis* datum" and believe that it is the level for the N.18/N.19 boundary and for the Miocene-Pliocene boundary. In contrast, Hays *et al.*⁴⁰ prefer to follow the criterion of Blow¹ in which the first evolutionary appearance of *S. dehiscentis* marks the base of zone N.19, and that the Miocene-Pliocene boundary is a little earlier than this level. They show that *S. dehiscentis* occurs at the bottom of core V24-59 and must have appeared earlier than 4.5×10^6 yr ago. Dymond⁴¹ reported a $4.3 \pm 0.3 \times 10^6$ yr K-Ar age for acidic volcanic glass occurring stratigraphically above N.18 material in core from the experimental Mohole⁴². Accepting the results of Hays *et al.*⁴⁰ and combining them with our Fijian data we infer that the Miocene-Pliocene boundary lies between 4.5 and 5.3×10^6 yr ago, and we suggest a best estimate of its age as $4.9 \pm 0.4 \times 10^6$ yr. Berggren¹⁷ adopts 5.0×10^6 yr as the age

of the boundary, based on estimates by Hays *et al.* for the time of first appearance of *S. dehiscentis* in deep-sea cores, which level Berggren refers to as the "*Sphaeroidinella dehiscentis* datum".

Future coordinated work in Fiji may make it possible to integrate for this period the planktonic foraminiferal zones with the East Indian letter stages, K–Ar ages for interbedded volcanics, and the geomagnetic polarity time-scale. Our dated samples are not pristine or numerous enough, the palaeomagnetic data are too limited, and our stratigraphic knowledge is not sufficient to make this correlation in detail at the present time.

Geologic Significance of the Fijian Volcanism

In Table 2 we present average compositions of volcanic rocks from the three Fijian volcanogenic units we have

Table 2 Average Chemical Analyses of Some Miocene to Recent Volcanic Rocks from Fiji

	Namosi Andesite formation	Koroimavua Volcanic Group	Mba Volcanic Group	Taveuni
N=	8	12	12	7
SiO ₂	58.4	51.0	51.0	48.3
TiO ₂	0.7	0.7	0.7	1.8
Al ₂ O ₃	17.4	18.2	16.2	17.5
Fe ₂ O ₃ *	7.0	8.7	9.6	10.0
MnO	0.2	0.2	0.2	0.2
MgO	3.4	4.3	6.2	7.1
CaO	7.5	9.6	9.5	10.5
Na ₂ O	3.9	3.4	2.8	3.3
K ₂ O	1.2	3.4	3.4	1.0
P ₂ O ₅	0.2	0.5	0.5	0.4
Rb	21.	60.	79.	21.
Sr	545.	1,270.	1,180.	474.
Ba	388	624.	680.	290.
Pb	4.	8.	9.	2.
Th	1.7	1.5	1.3	2.5
U	0.6	0.8	0.6	1.0
Zr	99.	48.	46.	142.
Hf	1.2	1.1	1.2	—
Ni	8.	13.	60.	104.
Co	19.	25.	33.	40.
Cr	19.	26.	148.	260.
Sc	18.	21.	26.	28.
V	192.	280.	27.	230.
Cu	78.	190.	162.	61.
Y	22.	19.	17.	33.
La	10.	12.	12.	—
Yb	1.4	1.6	1.7	—
⁸⁷ Sr/ ⁸⁶ Sr	0.7037	0.7041	0.7038	0.7035

* Total Fe as Fe₂O₃.

Summarized from analyses by Gill^{2,10,43}.

discussed. Gill⁴³ noted that volcanism on Viti Levu ceased to be predominantly intermediate in character at the end of the Miocene. During the time interval we have dated, volcanism changed from calc-alkaline (Namosi Andesite) to shoshonitic (Koroimavua and Mba Volcanic Groups). A similar change occurred elsewhere in Fiji on Vanua Levu and in Lau where Pliocene volcanism produced transitional olivine basalts. Table 2 also includes an average composition of olivine basalts from Taveuni where volcanism occurred as recently as 2,000 to 700 yr ago (E. Frost, personal communication).

Gill and Gorton⁴⁴ attributed this change in magma composition to alteration of plate configuration in the Fiji area. They argue that until the late Miocene the Lau-Colville Ridge (of which the Fiji islands are pinnacles) was underthrust from the east by the Pacific plate, but that at about the time corresponding to the Miocene–Pliocene boundary this subduction site began to migrate to the east away from Fiji. Karig⁴⁵ and Sclater *et al.*⁴⁶ have called attention to the youthfulness of the Lau Basin which today separates the Lau-Colville Ridge from the Pacific plate subduction site. DSDP drilling in 1971 has confirmed the Pliocene age of this basin⁴⁷.

Thus the change in magma composition shown in Table 2 accompanied trench migration and illustrates two effects of trench migration on the physical properties controlling magma genesis. First, volcanism became more basic (predominantly shoshonitic and basaltic instead of andesitic); second, the basic magmas changed from being initially shoshonitic (at least in places) to being nepheline-normative, lherzolite-bearing alkali basalts. The latter change brought with it higher concentrations in the alkali basalts of Ti, Zr, Nb, Hf, U, Th, and light REE, lower concentrations of K, Rb, Cs, Sr, Ba, and Pb, and higher Na/K ratios². These two changes are presumably due to the gradual cessation of underthrusting beneath Fiji and, therefore, to the removal of a source of heat as well as water and/or siliceous magma. Mantle presently beneath the Lau-Colville Ridge is seismically normal whereas that presently beneath the Lau Basin is characterized by low velocities and high attenuation⁴⁸. The change in magma composition may coincide with reversion of "anomalous" island arc upper mantle to "normal" intraplate upper mantle. In Fiji this change seems to have commenced shortly before the time corresponding to the Miocene–Pliocene boundary, and had a pronounced effect on the amount and composition of accompanying magmas.

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Comparison between Mice Chimaeric and Heterozygous for the X-linked Gene *tabby*

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A comparison of tail banding patterns between mice chimaeric and heterozygous for the X-linked gene *tabby* shows considerable differences between the two. The implications for the Lyon hypothesis of X inactivation and for clonal hypotheses of development are discussed.

ACCORDING to the single-active-X (or inactive-X or Lyon) hypothesis^{1,2}, every female mammal heterozygous for a gene carried on the X chromosome contains two genetically distinct cell populations, each expressing one only of the two contrasting alleles. The inactivation event which differentiates the two populations is postulated to be random, and much discussion²⁻⁷ has centred around the question of whether the orderly phenotype displayed by many of these heterozygotes is consistent with an initially random event.

Mice containing two genetically distinct cell populations, each expressing one only of two contrasting alleles, can be manufactured by the technique of embryo aggregation⁸⁻¹⁰. Here the existence of two populations is a matter not of hypothesis but of experimental design. If such an aggregation chimaera, with components carrying different X-linked alleles, were to display a phenotype like that of the corresponding heterozygote, it would show conclusively that the observed pattern could be generated by two populations of cells, and

the principle of scientific parsimony might suggest that two populations of cells existed in the heterozygote also.

We have therefore made aggregation chimaeras between strains of mice homozygous for the X-linked gene *tabby* (*Ta/Ta*) and for its normal allele (*+/+*), and compared them with heterozygous (*Ta/+*) females. *Tabby* was chosen because it has provided the subject-matter for much of the previous controversy^{4,6}, and because its phenotype in both heterozygous and homozygous (or hemizygous) condition has been exceptionally well described^{4,5,7,12}. The aggregants were mated to determine germ cell chimaerism; the gross appearance of the coat was recorded and photographed; and observations were made after death (as in 5,12) on the tail rings, the papilla vallata of the tongue, the Meibomian glands on the eyelids, and the plicae digitales on the paws. Control mice (*+/+*, *Ta/Ta*, *Ta/-*, *Ta/+*) were similarly studied. No study of the hairs was made, because this aspect has been investigated on a parallel series of aggregation chimaeras¹⁵. The teeth have been described elsewhere¹⁶.

Of the eleven adult mice derived from embryo aggregation, four showed no evidence of chimaerism in any of the characters examined. The other seven (two females, five males) were overt chimaeras of which two produced mixed progenies, three only + gametes and two only *Ta* gametes. In gross appearance, two out of seven were indistinguishable from *Ta/+* heterozygotes. Three others were mostly of heterozygous appearance, but two of these had an apparently homozygous "*Ta/Ta*" area, lighter in colour and lacking guard hairs, on one flank, while the third had a small area on the left side where the stripes appeared light on dark rather than *vice versa*, suggesting a higher concentration of *Ta/Ta* cells. One other

was mostly normal ("+/+"), but with a few definite tabby stripes on the back, and the last was mostly "*Ta/Ta*" but with some stripes on the back and a few macroscopically visible short hairs on the tail.

In tabby mice (*Ta*/–, *Ta*/*Ta*), the plicae digitales (transverse skin folds on the digits) and glandulae tarsales (Meibomian glands opening onto the eyelids) are absent or greatly reduced, while the papilla vallata on the tongue is represented only by a slit. Grüneberg¹² reported that the *tabby* gene was fully recessive with respect to the plicae digitales, in that the

ing tabby and wild-type control groups in respect of plicae digitales, papilla vallata and glandulae tarsales.

The chimaeras resembled the heterozygotes in having a reduced and variable number of glandulae tarsales, and in showing some individuals with the tabby condition of the papilla vallata, as well as some normals. The chimaeras differed from the heterozygotes, however, in showing a significantly higher incidence of reduced plicae digitales similar to those of the tabby controls or intermediate (thirteen out of twenty-four paws in six out of seven animals). In both

Table 1 Some Effects of the Tabby Gene in Normal Mice, Tabby Heterozygotes and Homozygotes, and Tabby↔Normal Chimaeras

Genotype	No. of animals	No. showing normal morphology for		Mean No. of glandulae tarsales	Mean No. of tail rings ± s.e.*
		Papilla vallata	Plicae digitales		
(C57BL × C3H/Bi) F ₁	7	7	7	10.9	77.96 ± 0.91
<i>Ta</i> ↔+ (wild-type)†	3	3	3	10.3	62.50 ± 0.87
<i>Ta</i> / <i>Ta</i> , <i>Ta</i> /–	12	0	0	0.0	74.70 ± 0.68
<i>Ta</i> ↔+ (tabby)†	1	0	0	0.0	72.50
<i>Ta</i> /+	12	6	5	4.7 ± 0.9	75.19 ± 1.37 ‡
<i>Ta</i> ↔+ (chimaeric)†	7	4	1	3.8 ± 1.8	70.69 ± 1.11 ‡

In order that all comparisons should be on an agouti background, embryos from the tabby stock maintained in this laboratory¹¹ were paired at the eight-cell stage¹⁴ with agouti (C57BL × C3H/Bi) F₁ embryos. Controls, which like the chimaeras were fully adult at the time of killing, included homozygous *Ta*/*Ta* females and hemizygous *Ta*/– males, heterozygous *Ta*/+ females, and +/+ (C57BL × C3H/Bi) F₁ animals.

* Tail rings were counted under a × 12.5 dissecting microscope. Four counts were made over a standard 3.5 cm length, two by one observer and two by another, and the results for each animal were averaged.

† Mice derived from embryo aggregation.

‡ For this comparison, $t_{17} = 2.55$, $P \sim 0.02$.

Table 2 Tabby Phenotype of 11 Mice derived from *T*↔+ Embryo Aggregation, Grouped into 5 Grades According to the Amount of Tabby Expression in the Coat

Phenotype of coat	No. of animals	No. showing normal morphology for		Mean No. of glandulae tarsales	Mean No. of tail rings	Germ cells*
		Papilla vallata	Plicae digitales			
Normal	3	3	3	10.3	62.50	+ (14, 24, 100)
Between normal and heterozygous	1	1	0	9.7	66.25	+ (86)
Heterozygous	2	2	1	2.9	70.50	{ + (10), <i>Ta</i> (50) <i>Ta</i> (26)
Between heterozygous and tabby	4	1	0	2.7	71.89	{ + (15, 17) + (1), <i>Ta</i> (8) <i>Ta</i> (46)
Tabby	1	0	0	0.0	72.50	<i>Ta</i> (15)

* Germ cells were classified from phenotypes of progeny from matings with *Ta*/*Ta*, *Ta*/– or +/+ partners. Numbers in parentheses indicate the number of germ cells of each type classified for each individual. This coincides with the total number of progeny for tested females, and the number of female progeny for tested males.

heterozygotes could not be distinguished from the homozygous normal animals. In our material (Table 1) the gene was not completely recessive with respect to the plicae: seven out of twelve heterozygous females showed some degree of tabby expression in seventeen out of their twenty-eight paws. The heterozygotes showed a very variable but always reduced number of glandulae tarsales, and for the papilla vallata, which was scored as an all-or-none character, some of the heterozygotes showed the normal and some the tabby condition. The four non-chimaeric aggregants resembled the correspond-

chimaeras and heterozygotes, the fore-paws more often showed reduced plicae than the hind paws. Variation in size of the plicae in the apparently "normal" chimaeras and heterozygotes was not measured. Table 2 gives data on the eleven aggregants arranged according to the phenotype of the coat.

The width of the tail rings was assessed by counting the number in a standard length (Table 1), and the distribution on the tail of scales, hairs and pigment was examined. The tabby heterozygotes did not differ significantly from the

homozygous normal group, nor from the tabby homozygotes. Grüneberg⁵ reported that $Ta/+$ heterozygotes had significantly more (that is, narrower) rings than normal mice; our failure to find a consistent difference probably reflects the difference in genetic background¹⁷ between the two studies. The mice derived from embryo aggregation showed significantly fewer tail rings than the heterozygotes ($P < 0.01$) or the homozygous tabbies ($P < 0.01$): the reduction in tail-ring number was shown most strongly by non-chimaeric aggregants (Table 2), so it cannot have been due to chimaerism. A maternal effect on tail ring width has been reported¹⁸⁻²⁰, so our result can probably be attributed to a uterine influence exerted by the Q-strain foster mothers. A similar maternal effect on number

heterozygotes ($Ta/+$) showed equally regular rows of scales (Fig. 1b); the hairs were thinner and less straight than in the normal animals, though still arranged in bunches of three; the pigment was less in amount and was restricted to the centre of the scale, leaving a pigment-free area at the anterior as well as the posterior border, so that the overall impression was of dark bands on a light background. Thus the heterozygous and homozygous normals both showed regular patterns, though they were readily distinguishable from one another. The tabby homozygotes ($Ta/-$, Ta/Ta), on the other hand, showed little or no regularity of pattern. The scales were arranged in rows, but these were irregular, and the pigment was widely and irregularly distributed (Fig. 1c), making the

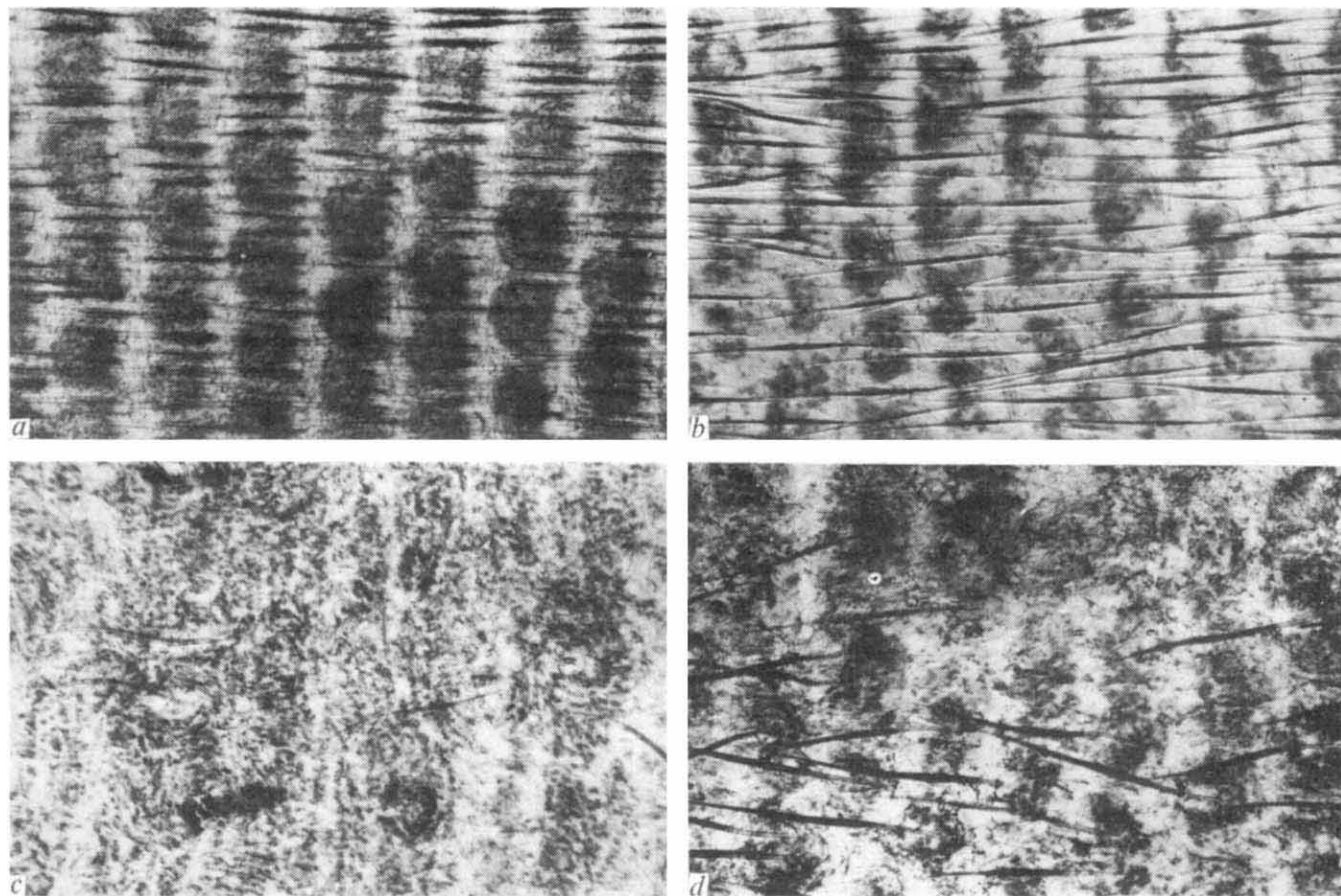


Fig. 1 Whole mounts of mouse tail skin. *a*, Normal $+/+$ (C57BL $\times$ C3H F_1). Regular bands of pigments and hairs. *b*, $Ta/+$ heterozygote. Less pigment, but bands regular. *c*, Ta/Ta homozygote. No regular bands, hairs sparse and abnormal. *d*, $Ta \leftrightarrow +$ chimaera. Patches of bald, band-free skin alternating with bands of pigment and hairs.

of lumbar vertebrae also proved to be exerted *via* the uterus^{21,22}. The normal genotype appears to be more susceptible than the tabby genotype to the uterine influence on tail ring width (Table 2: regression of tail ring number on coat phenotype grade, $b = 2.87 \pm 0.47$, $P < 0.001$).

The pattern of scales, hairs and pigment distribution showed striking and consistent differences between groups. The normal mice showed regular rows of scales (forming the tail rings), with long straight hairs growing in bunches of three, one bunch per scale (Fig. 1a). The pigment, contained in melanocytes, was distributed throughout the scale, except for a pigment-free area at the posterior border of each scale, giving an overall impression of light bands on a dark background. The tabby

tail rings hard to count. A few small single, twisted hairs were present, but most of the tail was bald. The non-chimaeric aggregants showed an identical appearance of scales, hairs and pigment to that of their respective control groups, suggesting that these features are not influenced by the uterine environment.

The tails of the chimaeras (Fig. 1d) differed strikingly from those of the heterozygotes. The overall banding pattern was much more irregular, and closer examination revealed a patchwork of "tabby" areas, with diffusely distributed pigment and few or no hairs, "normal" areas, with regular rows of scales showing pigment distribution and hair pattern as in homozygous normals, and in addition some "heterozygous"

areas, with pigment distribution closer to that seen in heterozygotes. A statistical analysis of densitometer tracings of the tail banding patterns is in progress.

Thus the manifestation of *tabby* in aggregation chimaeras bears no simple relation to that in heterozygotes. In the gross appearance of the coat pattern, the two were very similar, though there was a tendency for chimaeras to show more of the extreme forms of manifestation. This presumably reflects the fact that tabby and normal genes are present in equal numbers in heterozygotes, but may not be equally represented in chimaeras. Marked similarity between chimaeras and heterozygotes has been reported also for coat composition and detailed hair structure in respect both of tabby¹⁵ and of another sex-linked gene, greasy (*Gs*)²³. In respect of all-or-none, "threshold" characters (papilla vallata, glandulae tarsales, plicae digitales), the chimaeras resembled the heterozygotes in showing some degree of intermediate or variable manifestation, but at least in the case of the plicae digitales the expression of the tabby gene was considerably stronger in the chimaeras than in the heterozygotes. A similar tendency for dominance in heterozygotes but intermediate expression in chimaeras is seen in the molars of tabby mice¹⁶ and in some of the pleiotropic effects (for example on the xiphisternum) of the short ear gene²⁴. It may indicate that dominance is due to a diffusible gene product which can operate within the cell or between neighbouring cells to allow normal development, but that in chimaeras the "patch size" is too large to allow development to be fully normalized throughout "recessive" patches. Finally, the distribution of hairs and pigment in the tail was very different in chimaeras and heterozygotes.

Much confusion has surrounded the controversy over the activity of *X*-linked genes in mammals. Three quite distinct questions may be posed. (1) Is the single-active-*X* hypothesis correct, that is, is one or other *X* chromosome in many mammalian *XX* cells randomly and irreversibly inactivated during embryonic development? There seems wide agreement that the hypothesis is essentially correct, with some reservations as to whether inactivation is total or partial, and whether it may be partially reversed in later life. The most convincing evidence of its correctness comes from studies on cloned cells from females heterozygous both for an *X*-linked enzyme marker and for a cytologically identifiable *X* chromosome^{25,26}. (2) Are the orderly patterns (for example striping) seen in females heterozygous for *X*-linked genes consistent with the existence of two genetically distinct cell populations? The appearance of typical "heterozygous" tabby striping in the coat of *Ta/Ta* ↔ *+/+* chimaeras shows that such patterns can indeed be generated by two cell populations. (3) Are the patterns clonally related to the two genetically distinct cell populations, for example are the dark and light stripes due to the activity of *X*^{Ta} and *X*⁺ chromosomes respectively? This was the assumption which Lyon originally made¹: indeed, the existence of variegation in the coat of mice heterozygous for tabby and other *X*-linked genes was taken as evidence that two cell populations existed, and hence that one or other *X* chromosome was inactivated. It is this assumption that Grüneberg has chiefly questioned, seeking to interpret the patterns not in terms of local, autonomous gene action and clonal development ("cell heredity"), but as a manifestation of gene interaction, either within or between cells ("threshold phenomena"). In the case of the tabby coat, this question is still unresolved, but it must be remembered that the correctness of a hypothesis is in no way dependent on the correctness of the assumptions on which it was initially based. In the case of tabby tail rings, the evidence presented above makes it unlikely that the pattern seen in the heterozygote reflects the distribution of two genetically distinct cell populations. The bands of pigment are regular and of uniform width; the total amount of pigment is less than in either homozygote; hairs are distributed uniformly; no indication of a patchwork of the two homozygous phenotypes is seen. By contrast, the chimaera tails show an irregular appearance, with areas of bands similar

to those seen in the homozygous normals, interrupted by areas of pigment distributed randomly, with few or no hairs, as in the homozygous tabbies, and also areas of "heterozygous" pigment distribution.

If the tails of chimaeras and heterozygotes both contain two genetically distinct cell populations clonally distributed, why do the first show the patchwork appearance expected on the basis of local gene action, while the second show an integrated pattern suggesting interaction between cells? One explanation could be that the patch size is much smaller in heterozygotes, small enough to lie within the effective range of diffusion of the hypothetical gene product. Why should the patch size be smaller in heterozygotes than in chimaeras? This may indicate either that genetically similar cells tend to remain together in chimaeras, or that *X*-inactivation in tail epidermis takes place relatively late in development, after the main clonal relationships have been established. It has recently been pointed out²⁷ that cell mobility will have a much more disruptive effect on small clones than on big ones, since the rate of expansion of a clone increases exponentially with its size, while the rate of random cell movement does not vary with the size of the clone.

Although the occurrence of *X*-inactivation is now widely accepted, not much information is available as to whether it occurs in every tissue of the body, and if so, at what stage of development^{28,29}. In the case of retinal pigmentation, a recent study³⁰ has suggested that *X*-inactivation does not take place before 6 days *post coitum*, that is, 2 days after implantation. Further comparisons between heterozygotes for *X*-linked or *X*-translocated markers and corresponding aggregation chimaeras may throw light on these questions. Close similarity between the two, as is seen for melanocyte patterns in the coat¹⁵, may indicate early *X*-inactivation, or very thorough cell mingling in the chimaeras; divergence, as in the tail epidermis, argues for later *X*-inactivation in the heterozygotes, or for some degree of clonal cohesion throughout development in the chimaeras.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Evolution of the Pulsar Magnetosphere

BEFORE the discovery of pulsars, Hoyle, Narlikar and Wheeler¹ reasoned that the magnetic neutron star would have a very tenuous atmosphere. In 1969, Goldreich and Julian² reached a different conclusion. They argued that the electromagnetic forces near the surface of a rotating magnetic neutron star are so large by comparison with the gravitational forces that charged particles must be pulled out of the star at a sufficient rate for a dense magnetosphere to be generated.

More recent work by Ruderman³ and others⁴ on the physics of the neutron star surface suggests that both conclusions are likely to be right, but at different stages in the evolution of the star. If, as now seems probable, there is a sharp boundary at the star's surface, emission of charged particles depends on the appropriate work function and temperature of the surface (thermionic emission), or alternatively on the work function and electric field (field emission). The estimates of these quantities are such that both thermionic and field emission of electrons are expected to be negligible after approximately 10^8 yr. Field emission of positive ions may be ruled out. Thermionic emission of positive ions, or evaporation of neutral atoms which may subsequently be ionized, is likely to remain large for about 10^3 yr while the surface temperature remains $\geq 10^7$ K. After this time the surface temperature falls rapidly and the supply of positive ions will be greatly diminished.

These new results have an important bearing on the evolution of the pulsar magnetosphere⁵. A classification of the different stages of evolution based directly on the observational evidence has been given by Huguenin, Manchester and Taylor⁶, and a theoretical classification of the different possible types of pulsar rotating field has also been worked out⁷. Here I shall interpret the observational results in the light of the theoretical results in order to obtain more insight into the evolution of the pulsar magnetosphere.

During the first 10^3 yr, the supply of both electrons and positive ions from the surface of the star is expected to be large. In this phase therefore a dense hydromagnetic magnetosphere of the type studied by Goldreich and Julian², Sturrock⁸ and Mestel⁹ is likely to be generated. It is quite probable that pulsed emission at optical or higher frequencies will occur during these first 10^3 yr, but because of the high plasma number density in the magnetosphere, observable radio pulses are much less likely. With the exception of pulsars in binary systems, no pulsars of this type with clearly identified periods have been discovered so far. (It may be that some of these pulsars have magnetospheres which are so dense that their electromagnetic fields do not even remain stationary in the rotating frame, and this would make detection of their basic periods difficult.) Pulsars in binary systems (Cen X-3, Her X-1) may receive a copious supply of plasma from their companion stars which can heat the pulsar surface by particle impact¹⁰. Therefore they are quite likely to have dense magnetospheres and to behave like young pulsars, in spite of the fact that their real ages are $\geq 10^3$ yr.

The Crab pulsar has an age $\sim 10^3$ yr and is therefore in a state of transition. It has already started to produce radio pulses characteristic of pulsars older than 10^3 yr, but has not yet stopped pulsing at higher frequencies. Its braking index

is less than three because it still has a strongly non-vacuum magnetosphere¹¹. Any estimates of its surface magnetic field strength derived from estimates of its magnetic field strength near the velocity of light cylinder¹² must be raised to allow for the expected heavy attenuation of the field by the currents in the magnetosphere.

The hydromagnetic type of magnetosphere requires a minimum continuous flow of plasma from the star. This will not be maintained after about 10^3 yr because the rate of supply of positive ions will have fallen too low. (Clearly there cannot be a continuous loss of electrons unaccompanied by positive ions, as this would result in a continuously increasing electrostatic field.) Therefore the magnetosphere must acquire non-hydromagnetic features⁷, namely magnetic field lines which are pointing forwards at the velocity of light cylinder and zones where $E_{\perp} > cB$. Provided, however, that a reduced supply of positive ions is maintained, albeit at a rate much less than that required for a hydromagnetic field, $E_{\parallel}$ and charged particle drifts with respect to the rotating magnetic field lines will remain very small. Type S pulsars (simple integrated pulse profile)⁶, whose ages range from about 10^4 to 10^7 yr, are therefore likely to have non-hydromagnetic magnetospheres of this type.

After approximately 10^8 yr, all particle emission from the surface of the pulsar effectively ceases, the magnetosphere becomes a high vacuum, and radio pulses are no longer generated. Before this happens, the magnetosphere must develop appreciable $E_{\parallel}$, and, more important, significant charged particle drifts with respect to the rotating magnetic field lines. (For the vacuum field these quantities are large¹³.) When the charged particle drifts become significant but small, the pulsar is likely to change to Type D (drifting sub-pulse). When the drifts become large, the region from which the pulses are emitted probably becomes less well-defined, and the pulsar will then evolve to Type C (complex integrated pulse profile). Both these types are observed to occur predominantly among the older pulsars⁶.

It has already been proposed by Sutton *et al.*¹⁴ that drifting sub-pulses are caused by shearing of the plasma at the corotation break-up radius, that is, by plasma drift with respect to the rotating field. Their observational results strongly suggest that this is the case. At first sight, the fact that the sub-pulses are usually observed to drift forward through the pulse window, whereas the plasma is more likely to drift backwards with respect to the rotating field, argues against this interpretation. It is, however, quite likely that pulses emitted from the back of the emitting region will arrive at the observer before pulses emitted from the front of the region, thus producing an apparent reversal of direction of plasma drift through the region. This will happen if the azimuthal angle of pulse propagation from the back of the region is sufficiently in advance of that from the front of the region more than to compensate for the time taken for pulses to propagate across the region.

If this account of the evolution of the pulsar magnetosphere is substantially correct, then radio pulse emission mechanisms that do not involve emission of the pulses from near the velocity of light cylinder can be eliminated. According to the Sturrock radio emission mechanism⁸, for instance, the pulses are emitted from near the surface of the star. Furthermore, it assumes a hydromagnetic magnetosphere, which the new results for the surface physics rule out for all the radio

pulsars (apart from the Crab pulsar). The drifting sub-pulse phenomenon is very difficult to explain on the basis of emission from near the star surface or from the far field region. On the other hand, these drifting sub-pulses not only have a simple and reasonable explanation on the basis of emission from near the velocity of light cylinder, but also are observed to occur only in pulsars in the age range in which plasma drifts near the velocity of light cylinder are expected to occur.

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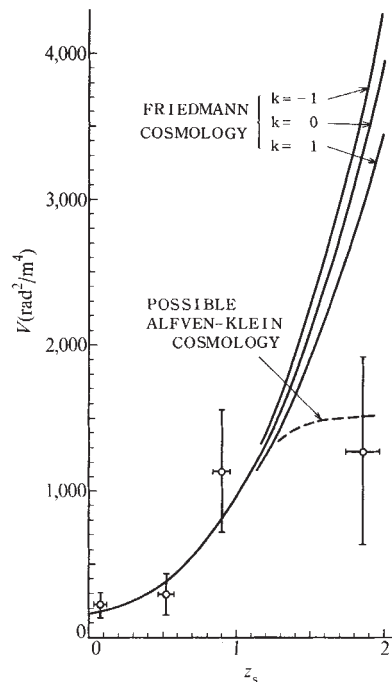


Fig. 1 The variance of the rotation measure, V (given for the observational data by the average of ρ^2 for each group of sources), as a function of the source redshift, z_s .

Faraday Rotation and Antimatter in the Universe

DATA on the Faraday rotation of polarized radio waves from extragalactic sources¹⁻⁵ have led to various interpretations⁶⁻⁹. For the general case of an ambiplasma (a plasma consisting of matter and antimatter mixed) and assuming that the plasma frequency is negligibly smaller than the wave frequency, the rate of Faraday rotation is proportional to $rB_{\parallel}\lambda^2$ (ref. 10), where r is the ambiplasma lepton residue ($r = n_e - n_{\bar{e}}$, with n_e = positron density and $n_{\bar{e}}$ = electron density), $B_{\parallel}$ is the component of the magnetic field parallel to the wave vector, and λ is the wavelength. For symmetric ambiplasma, $n_{\bar{e}} = n_e$, r , and therefore the rotation rate, vanishes. I consider the consequences of this for the interpretation of the astronomical data.

The origin of the rotation is still a matter of debate, and there are three possible regions where it might take place, for example, our Galaxy, the intergalactic medium between the source and our Galaxy, and lastly the source itself. Probably rotation occurs in all three. But if I consider only sources with galactic latitude, b , such that $|b| \geq 35^\circ$, following Fujimoto *et al.*⁶, then I expect that the contribution from our Galaxy will be minimized. In addition if the magnitude of the rotation measure ρ (defined by $\Delta\theta/\lambda_0^2$, where $\Delta\theta$ is the total Faraday rotation observed, and λ_0 is the wavelength observed at the Earth) increases strongly, on average, with increasing z_s , the redshift of the source, then the chief contribution to the rotation probably occurs in the intergalactic medium, although a z_s dependence of the source contribution cannot be ruled out. The averaged magnitude of ρ does seem to increase with z_s , and I therefore consider here only the contribution from the intergalactic medium, to determine to what extent it is possible to explain the z_s dependence of the observational data on the basis of this alone. I further assume that the intergalactic magnetic field has a random structure, that is, I do not consider

a homogeneous component of this field. Under this assumption a purely matter plasma in intergalactic space will cause Faraday rotations of alternating sign as the radio wave passes through regions of alternating sign of $B_{\parallel}$.

If the scale length of these magnetic field regions, or cells, obeys some probability law, then the polarization angle will undergo a one-dimensional Markov process similar to the random walk. The distribution of rotation measures for many sources at the same z_s should then be given by the normal distribution, and the variance of the distribution should increase with z_s . Assuming that $n_e \propto 1/l^3$ and $B \propto 1/l^2$, where l is the scale length of a cell, the variance as a function of z_s for the Friedmann cosmology (with matter only) is

$$V(z_s) = \gamma \left(\frac{2}{7} I_{7/2} + \frac{6}{5} \beta I_{5/2} + 2\beta^2 I_{3/2} + 2\beta^3 I_{1/2} \right) \quad (1)$$

where

$$I_a = (1 + z_s - \beta)^a - (1 - \beta)^a$$

$\beta = k/2$, $k = 0, 1, -1$ = the space curvature constant¹¹ and γ is a constant depending on l .

Observational values for the variance⁶ give 59 sources which satisfy $|b| \geq 35^\circ$, and I divide these into 4 groups defined by $z_s < 0.3$, $z_s = 0.3-0.7$, $0.7-1.1$, and $z_s > 1.1$; the groups have respectively 30, 9, 12 and 8 sources. In Fig. 1 I plot the average of ρ^2 for each group, corresponding to the variance, as a function of z_s , with the average value of z_s being taken for each group. The error bars in the vertical direction are the standard errors for the sampling averages, and those in the horizontal direction represent the errors due to the scatter of the sources over z_s within the group. To compare these with equation (1) I have fitted curves (one each for $k = 0, 1$ and -1) of the form $f(z_s) = V(z_s) + \chi$, with $V(z_s)$ given by equation (1), through the first three points in Fig. 1, using the method of least squares with γ and χ as fitting parameters. The resulting curves are shown in Fig. 1. For all the curves the observed value of the variance seems to be too low at $z_s = 1.85$, even taking into account the considerable errors involved.

One way of explaining this is to turn to the Alfvén-Klein cosmology^{12,13}. This cosmology incorporates the principle of matter-antimatter symmetry, and the situation envisaged at

the present era is that there are separated cells of pure matter and pure antimatter. Provided the cells are not such that $rB_{||}$ has constant sign, the combination of the matter-antimatter cells and the magnetic field cells, no matter what their respective scales are, will give rise to a Markov diffusion of the polarization angle similar to that with the magnetic field cells and matter only. So for small z_s the variance of ρ in the Alfvén-Klein cosmology will have a functional dependence on z_s given by equation (1). For large z_s , however (that is, for early times before the separation of matter and antimatter into different cells), the Alfvén-Klein cosmology envisages a state of uniformly mixed matter and antimatter, with r consequently zero; therefore $dV/dz_s=0$ for large z_s . The broken curve in Fig. 1 shows a possible $V(z_s)$ for the Alfvén-Klein cosmology.

Thus if I try to interpret the z_s dependence of ρ as being chiefly due to rotation in a random intergalactic magnetic field, the matter-antimatter symmetric cosmology offers the possibility of explaining the low variance of ρ at large z_s . But in view of the uncertainties in interpreting the Faraday rotation data, and also the small number of sources in the data groups, this evidence for the existence of antimatter in the universe, although intriguing, can hardly be regarded as conclusive.

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Endogenic Cratering Distribution on the Moon

WITHIN the cratering distribution for the Hyginus Rille floor, the chain craters represent a distribution which is distinctly different from that resulting by impact of interplanetary debris. It has been postulated¹⁻⁷ that Hyginus Rille was controlled in its development by tectonic activity along a graben zone, and some investigators^{1-5,7-10} have suggested that these chain craters are volcanic in origin.

Differences in cratering densities for a lunar rille floor and its adjacent mare may have relevance to understanding the origin of some rilles¹¹. High crater densities within regionally limited areas suggest that endogenic processes may supplement meteoritic processes for crater production. On Mare Cognitum¹² and the Apenninian debris surface¹³ high cratering

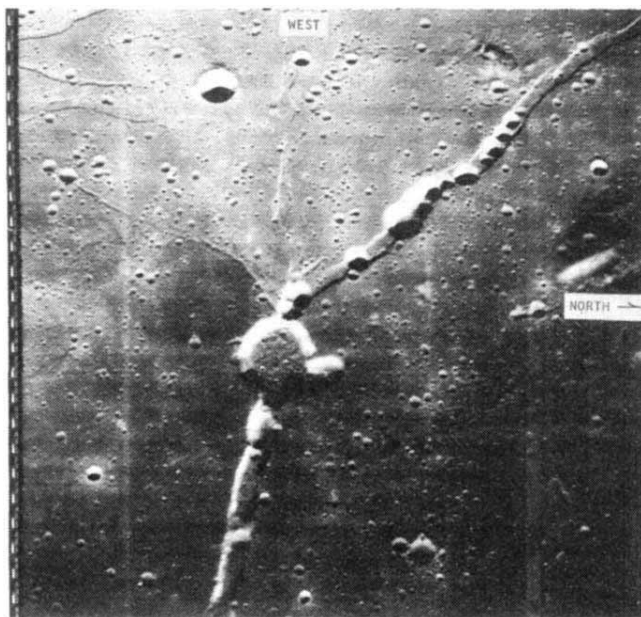


Fig. 1 A portion of Lunar Orbiter V Frame V-96M (framelets 269-246) showing the Hyginus Rille region. The largest crater, Hyginus (~10 km in diameter), is near the centre of the rille. Large low-rimmed chain craters such as the 13 in the northwest trending rille branch may have formed by endogenic mara-diatreme production processes analogous to some terrestrial craters.

densities probably represent secondary cratering effects. On the crater floors of Ptolemaeus¹⁴ and Copernicus¹⁵ and the interior terrace wall units of crater Copernicus¹⁶ cratering abundances have been attributed to igneous processes.

Cratering densities indicate relative age relationships between surfaces. It has been suggested^{9,12,13,17-20} that, in general, older surfaces display the greater crater density and include the largest craters. This age dating method assumes cratering originates chiefly by meteoritic impact.

I have made crater counts and measurements for the Hyginus Rille region from a medium resolution Lunar Orbiter V photograph (Fig. 1). Incremental and cumulative analyses of the

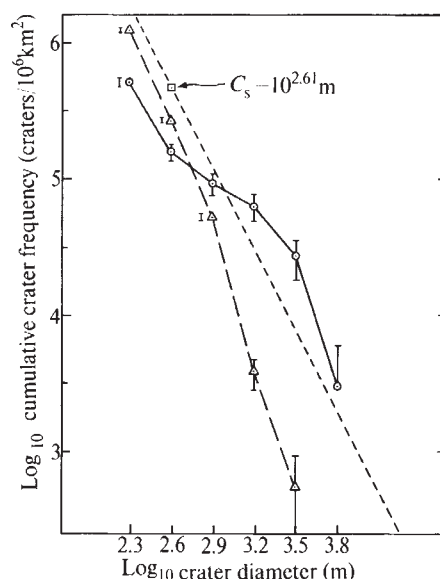


Fig. 2 Crater size-frequency distributions for the Hyginus Rille floor (○—○) and the adjacent mare (△—△) are normalized to an area of 10^6 km². Error bars on the curves represent a $\pm$ one sigma variation. The steady-state model distribution (---) $N=10^{10.9} D^{-2}$, where crater production and destruction rates are equal, permits deriving a relative age (C_s) for a surface.

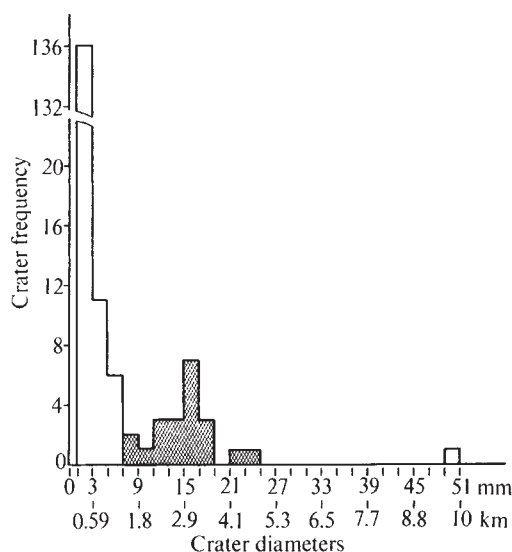


Fig. 3 Frequency of craters as a function of diameter on the Hyginus Rille floor. Shaded areas, the low-rimmed, deep-floored chain craters (these represent an endogenic size-frequency distribution which is differentiated from the predominantly meteoritic general crater distribution); white areas, smaller craters (0–51 mm).

crater-size frequency distributions show that the mare regions north and south of the rille are similar in age²¹. This adjacent mare surface has been identified as the Cayley Formation (Ica)⁵.

In Fig. 2 the relative age relationships alternate: for craters ~200–540 m the rille floor appears younger, and for craters >540 m it appears older. The inference that the rille floor is older than the mare is not supported by other observational evidence. Some chain craters exhibit dark-halo deposits which subdue mare features. In the northwest rille branch 10 of 13 chain craters cross-cut the mare. Infrared observations of the eclipsed Moon show Hyginus chain craters correlate with high thermal anomalies^{5,22}. These chain craters, although bouldered and fresh, cannot be Copernican age impact craters; their crater and rim morphology precludes this.

I assume a stochastic model for the formation and survival of lunar primary craters^{23,24} where infilling/obliteration produces an approximate Poisson distribution. Cratering densities for the rille and mare, compared as two Poisson distributed observations, show cratering differences which are statistically significant. For diameters ~790 m and >1,570 m, they are significant at the 98% and 99.9% level of confidence, respectively. The cratering differences indicate endogenic processes probably control the rille cratering excess.

When the steady-state curve²⁶, $N = 10^{10.9} D^{-2}$ (Fig. 2), is compared with an observed cratering distribution, $N = A D^B$, it defines two diameter-dependent distributions: a steady-state distribution where crater erasure equals crater formation, and a non-steady-state distribution, the large (L) crater "production"¹⁹ curve, $N_L = A_L D^{B_L}$, where craters are produced faster than they are erased. Intersection of the steady-state and production curves determines an equilibrium crater

$$C_s = (10^{10.9}/A_L)^{1/B_L - 1} \quad (-2)$$

which is a measure of relative^{17,27} age. For the adjacent mare large craters (786–6,280 m), $C_s = 407 (+60, -53)$ m, indicating that craters smaller than ~407 m are in steady-state. If a meteoritic origin for rille craters is assumed, then steady-state craters appear to be ≤ 8.7 km, but this is contrary to the evidence of the photographs. This implies that rille craters are controlled by selenological processes of non-impact origin. An Rb–Sr age ~3.65 Gy (ref. 28) and a $C_s = 141$ m (ref. 27) derived for the Apollo 11 site shows that the Cayley Formation at Hyginus is older^{21,29}.

In Fig. 3 two rille floor cratering distributions are distinguished: a Poisson type consisting of the smaller craters, and a Gaussian which is defined in a one-to-one correspondence with 21 chain craters as an endogenic size-frequency distribution. The endogenic craters are designated the Hyginus Formation low-rimmed crater materials (CEhy1). This unit is part of a rille system volcano-tectonic complex²¹ which includes the Hyginus Formation dome materials (domes, tumuli, and lava flows) and the Hyginus Formation bright deposit materials (fissure-aligned volatile/ash encrustations in subsided centres). They are late Eratosthenian or Copernican Period volcanogenic materials postdating the graben structure (Hyginus Formation, Elhy) of the rille.

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Properties of a Possible Superfluid State of ³He

RECENT results by Osheroff *et al.*^{1,2} on anomalies in the pressure-temperature relation and n.m.r. line of ³He along the melting curve have stimulated us to consider the possibility that the much discussed³⁻⁷ anisotropic superfluid condensation of liquid ³He may in fact be occurring. Briefly these results are: first, as the volume is varied continuously in time, the curve of pressure against time exhibits a discontinuous slope

at a point labelled A and a "glitch" at a point B, the slope before and after the "glitch" remaining about the same. Second, at point A the n.m.r. line starts shifting to higher frequencies until point B; it then returns to its unshifted value, but with a reduced intensity. The temperature at point A is estimated to be about 3 mK and at point B about 2 mK. If, as the nuclear resonance results show, the first anomaly is associated with the liquid, point A can be interpreted in terms of a second order phase transition in the liquid and point B, which also shows hysteresis effects, as a first order transition.

We show here that a superfluid condensation of liquid ^3He with a certain class of gap functions, $\Delta(\theta, \phi)$, must be accompanied by a spin precession of finite frequency at $k=0$ due to the dipole-dipole interaction among the ^3He nuclei. This would show up in the resonance experiment as a shift of the experimentally measured temperature dependence and order of magnitude. We argue further that, as the temperature is decreased, a second transition may take place into a class of $\Delta(\theta, \phi)$ for which there is no resonance shift. One such class would be one in which each spin component of the gap $\Delta^2(\theta, \phi)$ has cubic symmetry, but preliminary analysis shows that no such state is favourable for $l=1$, and is inconclusive as yet for $l=3$. We will mention another speculation on the B state later.

The physical reason for the n.m.r. shift is that in the presence of an interaction that does not conserve spin, like the dipole-dipole interaction*, the quasiparticles are linear combinations of up and down spin particles. As the dipole-dipole interaction is a second-rank tensor, its effect will not average to zero only if the density-density correlation function for a given spin combination is of less than cubic symmetry.

We show this by considering the Hamiltonian

$$H = H_0 + H_d \quad (1)$$

where H_0 includes the single particle energies, and also the interparticle interaction $V(r, r')$ which, if attractive in the l th particle wave, leads to superfluidity with an order parameter whose $\alpha\beta$ component in the spin space is

$$\Delta_{\alpha\beta}^{\alpha\beta}(r-r') = V_l(r, r') \langle \Psi_\alpha(r) \Psi_\beta(r') \rangle \quad (2)$$

In equation (2), $\Psi_\alpha(r)$ are the field operators for the α th z component of spin. H_d is the dipole-dipole interaction, which may be written as

$$H_d = \sum_{\alpha\beta\gamma\delta} \int dr' \int dr' W_{\alpha\beta}^{\gamma\delta}(r-r') \Psi_\alpha^\dagger(r) \Psi_\gamma^\dagger(r') \Psi_\delta(r') \Psi_\beta(r) \quad (3)$$

We now write equations of motion for $\Psi_\alpha(r)$, $\Psi_\alpha^\dagger(r)$ and linearize them by the Bogolubov method to obtain

$$i\hbar \dot{\Psi}_\alpha(r) = h(r) \Psi_\alpha(r) + \sum_{\beta} \int dr' V_l(r-r') \langle \Psi_\alpha(r) \Psi_\beta(r') \rangle \Psi_\beta^\dagger(r') + \sum_{\beta\gamma\delta} \int dr' W_{\alpha\beta}^{\gamma\delta}(r-r') \langle \Psi_\gamma(r) \Psi_\delta(r') \rangle \Psi_\beta^\dagger(r') \quad (4)$$

and similarly an equation for $\Psi_\alpha^\dagger(r)$, where the prime over the sum indicates that the states in $\langle \rangle$ must be taken to be singlet for even values of l and triplet for odd values of l . On using now the fact that $\langle \Psi_\gamma(r) \Psi_\delta(r') \rangle = -\langle \Psi_\delta(r) \Psi_\gamma(r') \rangle$ for even values of l (singlet states), it is seen that the last term in equation (4) adds up to zero. We may thus confine our attention to triplet spin state pairing.

In the standard fashion, we now use the transformation

$$\Psi_\alpha(r) = \sum_k u_{k\alpha} e^{ik \cdot r} v_{k\alpha} + v_{k\alpha} e^{-ik \cdot r} v_{k\alpha}^\dagger \quad (5)$$

and require that

$$[H, v_{k\alpha}] = -E v_{k\alpha}, \text{ and so on} \quad (6)$$

* We note that there may be a second spin non-conserving interaction of very roughly the same order of magnitude: the atomic-orbit-nuclear spin interaction. This interaction is of order 100 oersted for H_2 and may here be of the order of 1% of that number for two atoms in contact. It will be interesting when the experiments are better understood quantitatively to look for effects of such an L-I interaction. This interaction is discussed in, for example, ref. 8.

to obtain

$$E u_{k\alpha} \simeq \epsilon_k u_{k\alpha} + \sum_{\beta} \Delta^{\alpha\beta}(k) v_{k\beta} + \sum_{\nu\Delta\beta} D_{\nu\Delta}^{\alpha\beta}(k) v_{k\beta} \quad (7)$$

and similar equations for $v_{k\alpha}$. In equation (7) $\Delta^{\alpha\beta}(k)$ is the Fourier transform of $\Delta_{\nu\Delta}^{\alpha\beta}(r-r')$ and $D^{\alpha\beta}$ the Fourier transform

$$D_{\nu\Delta}^{\alpha\beta}(r) = W_{\nu\Delta}^{\alpha\beta}(r) \langle \Psi_\nu(r) \Psi_\Delta(r) \rangle \quad (8)$$

Next, we diagonalize the set of equations (7) and make the approximation that $D(k) \ll \Delta(k)$ to obtain

$$E^2 \simeq \epsilon_k^2 + \Delta_k^2 \pm [\Delta_k^\uparrow d_k^\uparrow + \Delta_k^\downarrow d_k^\downarrow + \Delta_k^\uparrow d_k^\downarrow + \Delta_k^\downarrow d_k^\uparrow] \quad (9)$$

where

$$d_k^{\alpha\beta} = \sum_{\nu\Delta} D_{\nu\Delta}^{\alpha\beta}(k)$$

In deriving equation (9), it has been assumed that $\Delta(k)$ is unitary. If $\Delta(k)$ is not unitary a spontaneous breaking of spin degeneracy is implied; we have not yet investigated the resulting n.m.r. behaviour for this case, which may well be the ground state for all odd $l \geq 3$.

The lifting of the spin degeneracy represented by equation (10) shows up in the n.m.r. signal as an average shift from the frequency $\omega_0 = \gamma H$ to ω given by

$$\omega^2 - \omega_0^2 \simeq 2\sum [\Delta_k^\uparrow d_k^\uparrow + \Delta_k^\downarrow d_k^\downarrow + \Delta_k^\uparrow d_k^\downarrow + \Delta_k^\downarrow d_k^\uparrow] \quad (10)$$

From equation (10) it is evident that because $W(k)$ is a traceless, symmetric second rank tensor, $\omega^2 - \omega_0^2$ is non-zero only if one component $|\langle C_{k\alpha} C_{-k\beta} \rangle|^2 (\equiv P_k^{\alpha\beta})$ is of lower than cubic symmetry. The observation of an n.m.r. shift would thus indicate such a paired state.

It is also seen that equation (10) is proportional to the square of the number of condensed pairs and hence to $T_c - T$ for T just below T_c . On using a dipole-dipole interaction of 1 gauss and estimating the other parameters from the Fermi energy of about 1 K, and the experimentally observed T_c of about 3 mK, we obtain

$$\omega^2 - \omega_0^2 \simeq 10^{10} \left(\frac{T_c - T}{T_c} \right) (c/s)^2$$

which has the right temperature dependence and the correct order of magnitude.

Now we discuss what angular momentum component the pairing may be in. The $l=1$ state does give the right order of n.m.r. shift. The correct $l=1$ state as derived by Balian and Werthamer⁷ is, however, one which gives the lowest free energy for all temperatures, and thus the B transition would be hard to understand on this basis. Further, it would give an n.m.r. shift at all temperatures.

Unfortunately the case for $l=3$ has not been worked out; similar things may in principle go on there. Our preliminary investigation shows, however, that with a unitary $\Delta(k)$ the "cubic" states that can be constructed from $l=3$ are not favourable. For $\Delta(k)$ not unitary, however, it is possible that "cubic" states of low enough free energy can be constructed to give the B phase. These would not have an n.m.r. shift using the mechanism discussed above and may have a sharply lower magnetic susceptibility than "unitary" states; however, we do not yet understand their other properties. Another possibility to account for the B transition is that the attraction parameter for $l=2$ is smaller than that for $l=3$ by less than 10%. As the free energy for $l=2$ drops faster than for $l=3$, this can account for all the features of the B transition—its first order nature, the disappearance of the n.m.r. shift, the decrease in the susceptibility, and so on. We doubt that values of l higher than 3 are involved, because the interaction parameter must fall off with increasing l . Earlier calculations³ showed $l=2$ to be the most promising. We suspect that $l=3$ wins because of exchange enhancement.

We now speculate on the static susceptibility as deduced from the strength of the n.m.r. absorption. In the "axial" state the susceptibility will in general be a tensor, and in an external magnetic field we expect the spins to be lined up to give the maximum susceptibility, just as, for example, in an antiferromagnet. The coercive field (~ 30 gauss) experimentally required before any n.m.r. shift is observed is $\approx \sqrt{H_d \Delta}$, very reminiscent of a spin flip transition in an antiferromagnet. We suspect that the strength of the n.m.r. absorption reflects the perpendicular susceptibility, which we expect to be nearly temperature independent if the state is like the equal spin pairing state that Anderson and Morel⁶ investigated for $l=1$. One possible way to test for such a state is to vary the angle of the polarization of the r.f. field in the n.m.r. experiment with respect to the d.c. field. On the other hand, in a "cubic" phase, the susceptibility tensor will be isotropic; hence, we might expect a drop in the susceptibility at B. The magnitude of the drop may crucially depend on the fact that if χ_0 , the "single particle" susceptibility, drops, then χ , the "many body" susceptibility, drops even more.

As there is a situation of broken symmetry, one would expect in general to have domains formed. The field of 30 gauss probably aligns the domains. The non-equilibrium effects reported by Osheroff *et al.* may be associated with domains.

The behaviour of dP/dt at A reported by Osheroff *et al.*¹ is understood if the jump in the specific heat at T_c is about a factor of two and we assume that the solid is not in thermal contact with the liquid. This is the right magnitude for the jump in the specific heat for the $l=3$ transition.

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Subduction and Aseismic Ridges

WHETHER or not aseismic ridges and seamounts on the ocean floor are due to stationary hot spots¹⁻⁷ or some other process, one may ask what happens when an aseismic ridge is forced into a subduction zone. Here I consider one aspect of the problem, namely whether or not the plan view of island arcs and their associated trenches has been influenced by aseismic ridges being subducted. Island arc type subduction occurs primarily in the western Pacific and the Indian Oceans. As indicated in Fig. 1, there are a small number of aseismic ridges in the vicinity of the north and western Pacific and Indian trenches. The bathymetric charts recently published by Chase *et al.*⁸ were used to obtain the general outlines of the Pacific aseismic ridges. The two most massive Pacific ridges, the Emperor and Caroline, intersect the trench system near prominent cusps, where one arc gives way to the next. In

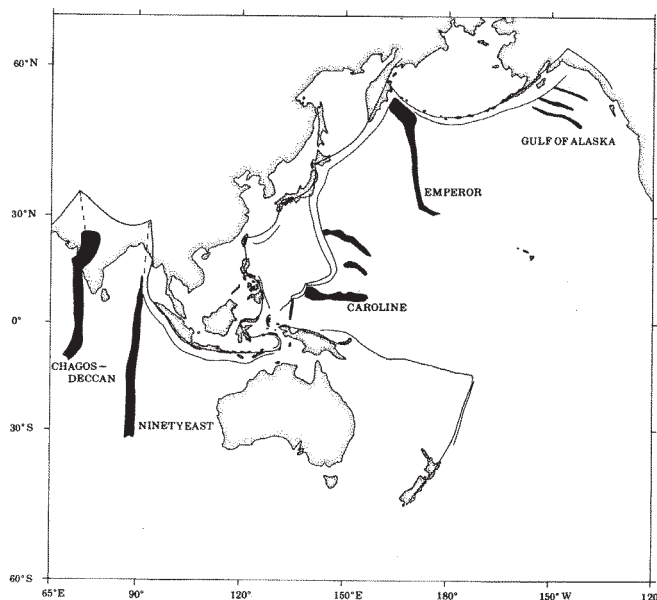
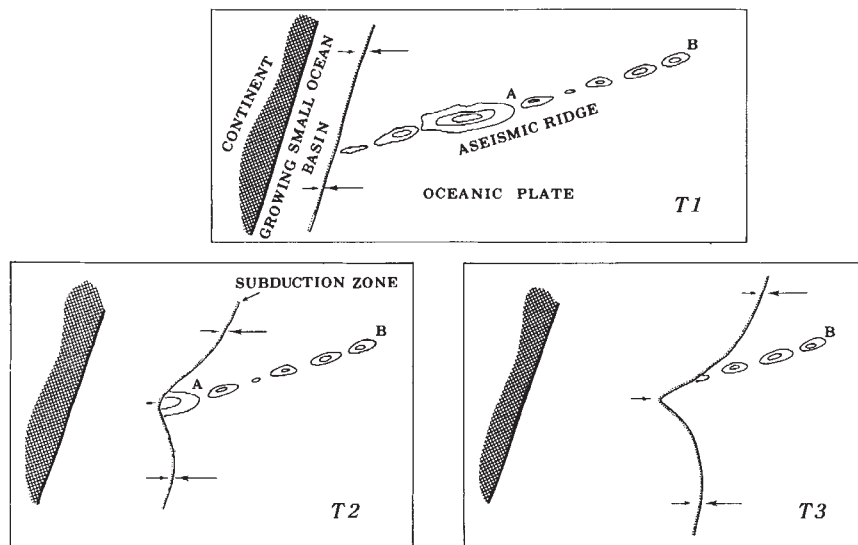


Fig. 1 Simplified sketches of major aseismic ridges and seamount chains near island arcs of the Pacific and Indian oceans, based on standard bathymetric charts and the new north Pacific atlas⁸. Plate collision boundaries shown by single or double lines, the latter showing both volcanic arc and trench. Marcus-Necker Ridge intersects the northern Marianas arc between the Emperor and Caroline ridges. The Deccan Traps are considered part of Chagos-Deccan volcanic line⁹.

addition, the Marcus-Necker ridge intersects the northern edge of the arcuate portion of the Marianas trench; the Gulf of Alaska seamounts, presumably generated by the Juan de Fuca and/or its neighbour, the Yellowstone plume⁵⁻⁷, intersect the Aleutians only slightly west of the cusped eastern end of the Aleutian arc. In the Indian Ocean, the two principal cusps in the boundary between Asian and Indo-Australian plates are now intra-continental. It may reasonably be supposed, from its arcuate character and continuity with the Indonesian arc, that the Himalayan arc replicates the shape of an extinct island arc system lying in the Tethyan Ocean, that lay between India and central Asia until it was closed by subduction in later Tertiary times. Whether or not the Himalayan arc preserves the shape of an old island arc, the Indian Ocean's two prominent aseismic ridges⁹ extend, with comparatively minor extrapolation, into the two major cusps of the Himalayan tectonic arc.

The number of examples are small—a typical predicament for observations involving features of continental dimensions. But it seems unlikely to me that the coincidence of cusps with aseismic ridges is mere chance. Therefore, I suggest the qualitative model of Fig. 2. Inner arc spreading, probably episodic, causes the volcanic arc to migrate away from the continent¹⁰⁻¹², while oceanic crust, carrying aseismic ridges as passive cargo, is subducted according to the now-conventional model. When a massive segment of an aseismic ridge enters the trench, however, inner arc spreading is slowed by the positive buoyancy of the volcanic pile, which resists subduction. The subduction boundary thus acquires a cusp; a number of such cusps produces a series of arcs. Because plumes or hot spots start, fluctuate in discharge¹³ and presumably die, there is no necessity that a cusp must have a massive aseismic ridge going into it at present. Even if aseismic ridges did not thicken and thin, there is no reason why a ridge should parallel the vector of relative motion between island arc and oceanic plate: if it is not exactly parallel, the present intersection of island arc with aseismic ridge may be displaced from where it was when the cusp was created (Fig. 2). Conversely, however, the hypothesis demands that where a massive aseismic ridge intersects an island arc system, there should be a cusp; the data of Fig. 1 suggest that this is so. In the case of the Pacific

Fig. 2 Diagrammatic explanation for coincidence between aseismic ridges and island arc cusps. Owing to buoyancy of lithosphere containing massive, relatively low density segment of aseismic ridge, inner arc spreading is slowed. An aseismic ridge will not, in general, parallel the subduction vector. Chronological sequence $T1 \rightarrow T2 \rightarrow T3$.



arcs, the kinetics of late Tertiary plate motion has been such¹⁴ that aseismic ridges are nearly parallel the vectors of relative motion. Were this not the case, the juxtaposition of cusps and ridges would only hold for the very recent geological past, and the cusps and arcs would have had to acquire their shape very recently for the hypothesis to hold. The Marianas arc acquired its present shape since the Pliocene¹⁰⁻¹²: there has thus been little opportunity for the aseismic ridges to move away from the cusps. Exactly when the other arcs acquired their present curvature is still unknown.

In the case of the Chagos-Deccan and Ninety East aseismic ridges the hypothesis (Fig. 2) demands that northward extensions of these hot spot trails existed in the now-closed Tethys, and that the Indo-Australian plate has moved essentially due north since the time their extensions were subducted. The assumption of such persistent northward motion finds strong support in palaeomagnetic¹⁵ and plate tectonic¹⁶ evidence, but the hot spot/plume concept¹⁻⁷ uses the aseismic ridges themselves as convincing evidence. If Asia is taken to be nearly fixed with respect to the mantle, to first order, the Ninety East and Chagos-Deccan aseismic ridges record not only the motion between the Indo-Australian plate and the mantle, but also the motion between this plate and Asia. The Chagos-Deccan lineament shows that northward motion has continued from about 60-65 m.y. BP, the age of the Deccan Traps¹⁵, to later Tertiary times. Only the southern to central parts of the Ninety East Ridge are as young as the Deccan Traps; the northern surviving limit is Campanian or older¹⁷. So it seems that the aseismic ridges have roughly paralleled the relative plate motion vectors at the cusps of the Himalayan arc for at least 80 m.y. It can therefore not be argued that the location of the cusps on the extensions of the two aseismic ridges is an accidental arrangement of recent geological time, postdating the formation of the arcs.

The hypothesis that aseismic ridges inhibit inner arc spreading (Fig. 2) depends on whether or not the ridges have enough positive buoyancy.

The more massive ridges have widths of ~50 to 500 km; the excess crustal thickness is generally supposed to be in the form of basalts and intrusives of basaltic composition deposited on pre-existing oceanic crust. In the case of the aseismic ridge produced by the Iceland plume, crustal thickness varies from about 8-15 km on Iceland¹⁸ to 15-20 km near the Faeroes Islands¹⁹. The Hawaiian ridge generally rises 3 to 10 km above the average top of the oceanic crust. At three relatively massive portions of the ridge—Hawaii, Oahu, and Gardner Pinnacles—geophysical data indicate that the crust is thickened about 10 km, of which 2 km is attributed to subsidence and 8 km to transformed upper mantle²⁰. Detailed refraction

measurements in the south of Hawaii—close to the present plume position—showed crustal thickness varying from 12 to 17 km (ref. 21). In summary, the normal oceanic crust is augmented by about 10 km basaltic crust, of average density no more than 3.0-3.1 gm cm⁻³, in the more massive portions of aseismic ridges. If the subcrustal portion of the lithospheric plate has a density 3.3 gm cm⁻³, the average total lithospheric density is decreased by 0.02 to 0.03 gm cm⁻³ for aseismic ridge densities of 3.0 to 3.1 gm cm⁻³. For an upper mantle density of 3.4 gm cm⁻³ the density decreases are 0.03 to 0.05 gm cm⁻³. At present the process of subduction and inner arc spreading are not understood sufficiently well to decide whether density differences of this order are significant. But the differences could locally overcome the negative buoyancy of the colder subducted slab, which is held to be a partial driving force for sea floor spreading²². The models calculated by Minear and Toksöz²³ show that for subduction velocities near 1 cm yr⁻¹, a zone interior to the downthrust slab and extending from about 100 km to 300 km in depth is 0.01 gm cm⁻³ denser than the surrounding mantle. For a subduction speed of 8 cm yr⁻¹, an 80 km thick interior zone extending from depths of 80 to 500 km is about 0.04 gm cm⁻³ denser. These density excesses are similar in magnitude to average-density deficits of 0.02 to 0.05 gm cm⁻³ due to a 10 km thick aseismic ridge riding on a normal oceanic plate. The density excess applies to the whole downgoing plate, whereas the aseismic ridge is only 50 to 500 km wide; however, broader density anomalies of the subcrustal hot spot lithosphere cannot be ruled out.

As far as previous explanations for the curvature of island arcs are concerned, relatively few students of subduction zones seem to have considered this problem. An example of a pre-sea-floor spreading explanation is Lake's suggestion^{24,25} that plane fractures intersecting a spherical Earth would produce circular arcs on the surface. A more recent argument²⁶ is based on the proof that a flexible, inextensible, thin spherical shell may be bent inwards through an angle θ only on a circle whose radius of curvature is $1/2 \theta$. The data supporting this are: the downthrust lithospheric slab, thin compared to the radius of the Earth, typically dips 45° into the mantle; and island arc curvature radii—expressed in angular measure on the sphere—are typically one half this value. But the Tonga-Kermadec "island arc" and the subduction zones on the west coast of south and central America are either straight or concave seaward. The Marianas arc has acquired its curvature in later Tertiary times¹⁰⁻¹²; its ancestor, the Palau-Kyushu Ridge, was essentially straight. It follows that the curvature of the island arcs evolved gradually, and the bent sphere model²⁶ is unrealistic. There may nevertheless be a minimum-energy consideration that tends to make straight "arcs" evolve

into curved ones. Even if this is so, some irregularity is needed to make cusps form at one point rather than another, and aseismic ridges may serve in exactly that capacity. Major fractures in the oceanic crust might be proposed as another such irregularity; however, none of the definite examples of fracture zones being subducted under island arcs seem to have influenced the curvature of the arcs.

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A Topographic Interpretation of the Mathematician Ridge, Clipperton Ridge, East Pacific Rise System

WITHIN the framework of plate tectonics, dating of the ocean floor has been almost exclusively achieved by studies of magnetic anomalies. But a north-south striking mid-ocean ridge system generating new seafloor in the equatorial region produces magnetic anomalies which are of small amplitude and are frequently distorted and disguised by magnetic "noise". The correspondence between the absolute depth of oceanic crust and its age¹ has provided an alternative method of age dating the oceanic crust anywhere in the world's oceans, provided, of course, that the crust was generated by a seafloor spreading system. Such a correlation tool has been used on the East Pacific Rise (Fig. 1) between 20° N and the equator¹ and between 0° and 20° S (ref. 2).

This method assumes that the lithospheric plate is isostatically compensated. All elevation is thus caused by thermal expansion as new hot crust is added to the plate at the ridge crest and thermal contraction as the newly solidified crust spreads from

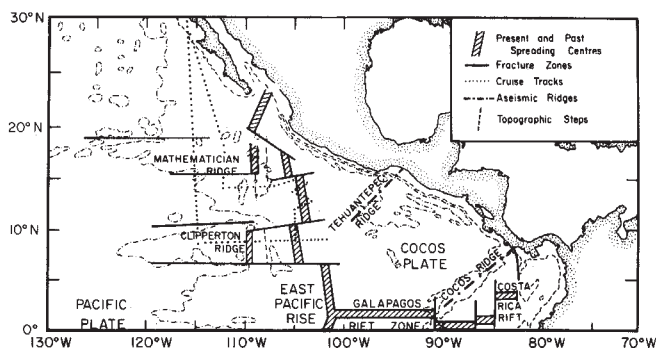


Fig. 1 Physiographic diagram of the central eastern Pacific. The light dot-dash line is the 4,000 m contour.

the ridge axis and cools. A remarkable correspondence of age to depth was found in the Pacific, Atlantic, and Indian Oceans¹.

Sclater *et al.*¹ presented a tectonic interpretation of the East Pacific Rise system north of the equator based upon this age-depth relation. They found that the East Pacific Rise is a relatively young feature, the seafloor spreading centre having jumped from a fossil ridge crest to the west of the present axis approximately 4 m.y. BP. The boundary between the fossil system, the Mathematician Ridge, and the East Pacific Rise is marked by a distinct topographic step caused by an age discontinuity across the step. They described the Mathematician Ridge as an approximately north-south ridge extending from 18° N to 12° N having an average crestal depth of 3,200 m and smoothly deepening slopes characteristic of a mid-ocean ridge system. They also postulated the existence of another fossil ridge, the Clipperton Ridge, striking north-south from 10° N to 8° N. Herron³, using magnetic anomalies to interpret crestal age, found anomaly 5 symmetrically distributed about the East Pacific Rise at 14° N. She thus concluded that magnetic data do not agree with the topographic age of the axial shift, but rather support a radical shift in the position of the axis between 10 and 15 m.y. BP.

Anderson and Sclater² used the depth/age method to interpret the tectonics of the East Pacific Rise from the equator to 20° S. They found a similar fossil ridge called the Galapagos Rise, on the Nazca Plate, and postulated that a jump in the spreading centre had occurred ~6 m.y. BP to the present East Pacific Rise system. Herron³, again using magnetic anomalies, found that the Galapagos Rise had jumped 10 m.y. BP.

Sclater *et al.*¹ used the north Pacific to construct their topography/age curve by correlating crestal depth with the well-known magnetic structure from Atwater and Menard⁴. The depth-age relationship is thus based upon magnetic anomalies. It is therefore extremely disturbing to find the seeming disagreement between the two methods in the age of the axial jump in the eastern Pacific.

In an attempt to shed light upon the solution to this problem, two long east-west topographic and magnetic lines were run across the Clipperton Ridge-East Pacific Rise at 8° N and the Mathematician Ridge-East Pacific Rise at 14° N on RV Thomas Thompson cruise TT-65 in November 1971. The results of these lines clearly show the compatibility between the magnetic and topographic interpretations of the Mathematician Ridge, Clipperton Ridge, and East Pacific Rise system.

A jump in spreading centre is essentially a perturbation of the plate tectonic model. The dependence of depth on age away from spreading centres theoretically requires that such a jump produce a step or ramp in the topography of the ridge². The depth of the top of the step should correspond to the depth of the crest of the fossil ridge, because both are the same age, and the depth of the bottom of the step should correspond to the age of crust into which the new spreading centre jumped. The observation of such steps on the flanks of the East Pacific Rise indicates that the lithospheric plate model's assumptions

of vertical cooling and intrusion are valid on the East Pacific Rise system.

The profile across the Clipperton Ridge–East Pacific Rise at 8° N shows a clear step in the topography between the two ridges (Fig. 2). Papagayo I profile from Scripps Institution of Oceanography at 9° N substantiates this interpretation. The depth of the East Pacific Rise crest is 2,700 m. The depth of the Clipperton Ridge crest and the top of the step is 3,500 m, a depth corresponding to an age of 7.5 m.y. (ref. 1). The depth of the bottom of the jump is 3,750 m corresponding to an age of 12 m.y. BP. The magnetics are uninterpretable except for the central anomaly on the East Pacific Rise crest. Thus the spreading centre jumped from the Clipperton Ridge 220 km to the east at 7.5 m.y. BP to begin spreading in 4.5 m.y. old crust.

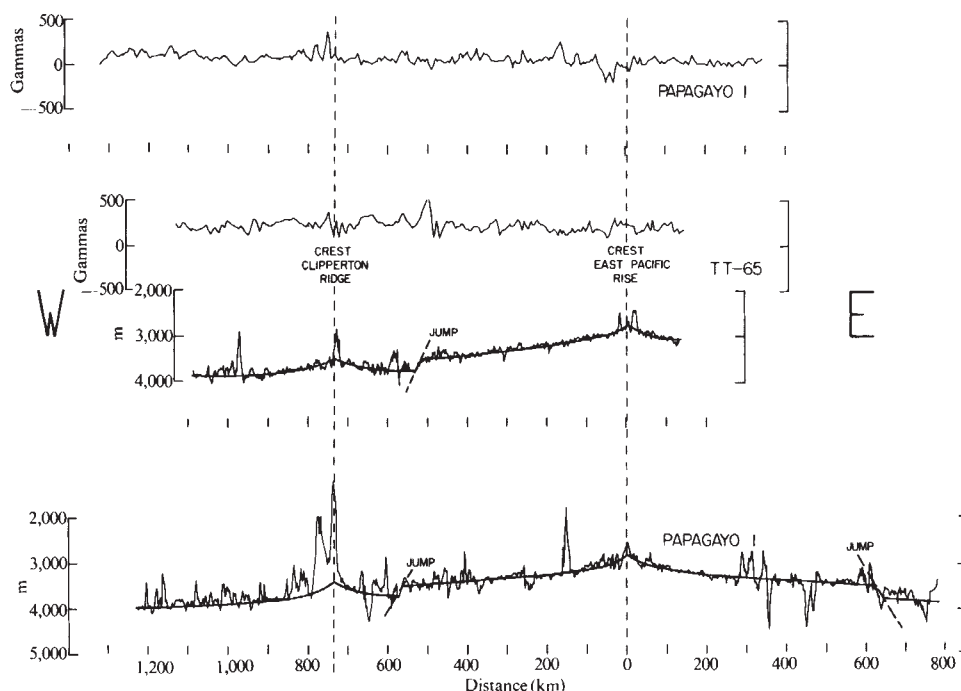
province. This step occurs at 15° 15' N. Ages across this step jump from the 4.5 m.y. old crust of the fossil ridge to 12 m.y. old crust generated originally on the East Pacific Rise.

Thus, Herron³ found anomaly 5 associated with a section of the East Pacific Rise which did not jump when the Clipperton Ridge centre jumped 7.5 m.y. BP to the present East Pacific Rise or when the Mathematician Ridge jumped to the present system 4 m.y. BP.

The Mathematician Ridge system extends only from 18° N to 15° 15' N, and its termination corresponds to a dramatic "kink" in the 4,000 m contour toward the East Pacific Rise crest at this latitude (Fig. 1).

The disagreement between topographic and magnetic interpretations of the East Pacific Rise system north of the equator is thus totally resolved. Further, this adjustment in the

Fig. 2 East-west bathymetric and magnetic profiles at 8° N (TT-65) and 9° N (Papagayo I) across the Clipperton Ridge and East Pacific Rise. The solid line is the best fitting topographic curve through the data.



Spreading rates can be determined from the slope of the topographic profile and have been demonstrated to be accurate to $\pm 10\%$ by Anderson and Sclater². The 8° N profile indicates that the East Pacific Rise is spreading at 6.5 cm yr^{-1} half-rate, and the Clipperton Ridge was spreading at 4.0 cm yr^{-1} half-rate before the jump.

The profile between the Mathematician Ridge and East Pacific Rise at 14° N shows extremely rough topography across the Mathematician Ridge but no topographic step or smooth slope away from the crest. Rather, the crust to the west of the Mathematician Ridge has the proper depth to be generated from the East Pacific Rise. In other words, there is no indication that the rough topography is a fossil spreading centre and no evidence that a jump has occurred at 14° N. Further, the magnetic anomalies are identifiable through anomaly 5 and indicate a spreading rate of 4.6 cm yr^{-1} half-rate.

Sclater *et al.*¹ show that the Mathematician Ridge at 16° N has smooth, continuously deepening slopes. To examine further the clear difference in physiography between the seamounts at 14° N and the ridge at 16° N, we ran a north-south seismic reflexion profile on the flanks of both features from 18° N to 14° N (Fig. 3). The topography drops from an unsedimented shallow northern section which is the flank of the fossil spreading centre to the well-sedimented flanks of the seamount

Mathematician Ridge system gives an even better fit of the Cainozoic reconstruction of the East Pacific Rise presented in Sclater *et al.* (ref. 1, Fig. 14) to the observed topography.

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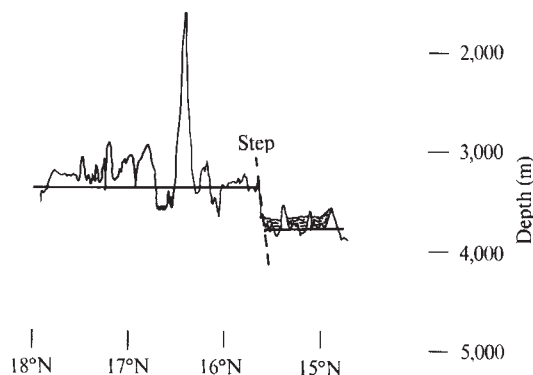


Fig. 3 North-south seismic reflexion profile run along 112° W longitude. Generally $< 20 \text{ m}$ of sediment exists north of the step, and up to 125 m is found to the south. Solid line is best-fitting topographic line through data.

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Infrared Magnetic Circular Dichroism in the Study of Metalloproteins

MAGNETIC circular dichroism (MCD) is the differential absorption of left and right circularly polarized light induced by a magnetic field¹⁻⁴. By contrast with natural circular dichroism (CD), MCD is a property of all materials. Until now no instrumentation has been available for measuring CD and MCD in the near infrared (IR) spectral region with the high sensitivity routine in the visible and near ultraviolet. Some CD studies at wavelengths greater than 1 μm have been made on iron-sulphur proteins^{5,6}. No MCD measurements have been made in this region. The recent development of photoelastic modulators (PEM)⁷ has made this possible and at the University of Southern California we have built a near IR CD and MCD instrument incorporating an 'infrasil' quartz PEM⁸. The instrument has a sensitivity of close to 10^{-5} absorbance units over its spectral range of 800–3,000 nm and employs magnetic fields up to 60 kgauss. We believe this will permit important new studies of metalloproteins and here report initial results on haemoproteins.

Most molecules of biological interest exhibit no electronic absorption, and hence no CD or MCD, in the near IR. However, in proteins containing transition-metal ions electronic absorption bands commonly occur in the near IR. The study of these bands is often useful in elucidating metal-ligand stereochemistry and electronic structure (refs. 5, 6, 9, 10 and Makinen and Eaton, unpublished work). Almost all haemoproteins exhibit near IR absorption bands and, of the entire electronic spectrum, these are the most sensitive to metal coordination, oxidation state and spin state^{10,11}. The near IR bands of ferric derivatives have recently been used by Perutz *et al.*^{12,13} to investigate haem-haem interaction in ligand binding by mixed-oxidation-state haemoglobins. The assignment of these bands is necessary for the detailed interrelation of spectra and structure. Assignments have previously been suggested for the ferric spectra^{11,14} but not firmly established. We have made CD and MCD measurements on two high-spin ferric compounds [acid methaemoglobin (metHb) and acid metmyoglobin (metMb)] and two low-spin ferric derivatives [cyanomethaemoglobin (metHbCN) and cyanometmyoglobin (metMbCN)]. The MCD of the low-spin near IR bands provides definite evidence in favour of a porphyrin-to-iron charge-transfer mechanism for these transitions. The shape of the MCD is quite different for the high-spin bands and MCD should therefore be a much more diagnostic tool than absorption spectroscopy for studying spin-state equilibria in ferric haemoproteins.

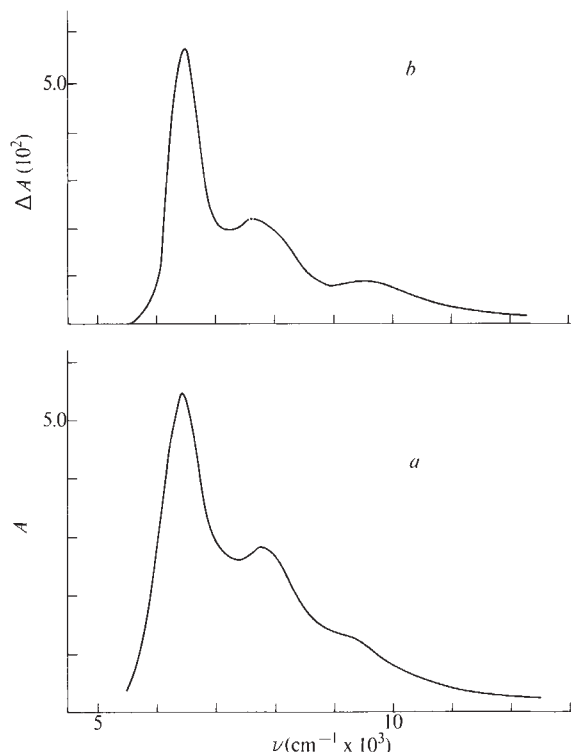


Fig. 1 Near infrared MCD and absorption of human cyanomethaemoglobin in D_2O (1.0 cm pathlength, 0.0013 M in haem, 0.1 M potassium phosphate buffer, $\text{pH}=6.6$, room temperature). Oxyhaemoglobin, prepared according to Perutz¹⁷, was oxidized with excess potassium ferricyanide, passed through a 'Sephadex G-25' column, and dialysed against a 0.1 M potassium phosphate buffer in D_2O for three days. The cyanide complex was formed by the addition of excess solid potassium cyanide. *a*, Absorption spectrum measured with a 'Cary 14' spectrophotometer. Reference is 1 cm path of dialysate. *b*, MCD at $H = +39.2$ kG. $\Delta A \equiv A_L - A_R$, where L and R denote left and right circular polarizations, respectively, and *A* is absorbance. Spectral bandwidth is ~ 4 nm, and time constant 3 s.

The absorption spectrum of a room-temperature solution of human metHbCN in the region 800–2,000 nm ($12,500\text{--}5,000\text{ cm}^{-1}$) is shown in Fig. 1*a*. The spectrum of sperm-whale metMbCN is very similar. We have measured the CD and MCD of these solutions at room temperature in the region 800–2,000 nm, the latter with magnetic fields up to 50 kG. Typical spectral bandwidths and time constants of ~ 4 nm and 3 s provide a CD sensitivity close to 10^{-5} absorbance units. Without magnetic fields we observe no CD due to the IR transitions. On the other hand a large MCD is observed, as shown in Fig. 1*b* for metHbCN. The MCD of metMbCN is very similar.

To interpret the spectra we use a molecular orbital model. The highest occupied orbitals are nearly three-fold degenerate iron d orbitals, belonging to t_{2g} and b_{2g} , e_g in O_h and D_{4h} symmetries respectively. In the ferric, low-spin cyanide derivatives, five electrons occupy these orbitals in the lowest electronic configuration and lead to three close-lying Kramers doublets whose spacing and wave-functions depend sensitively on the d orbital splittings and spin-orbit coupling. Porphyrin π -electron theory¹⁵ predicts the highest filled orbitals to be two, nearly-degenerate orbitals belonging to a_{1u} and a_{2u} in D_{4h} symmetry. Excitations from these orbitals into the d shell hole lead to two charge-transfer states, ${}^2A_{1u}$ and ${}^2A_{2u}$ in D_{4h} symmetry, transitions to which are fully allowed. In the porphyrin-to-iron assignment, the near IR bands are thus attributed to these two, nearly-degenerate transitions. The width ($\sim 5,000\text{ cm}^{-1}$) of the absorption appears too large for one electronic transition alone, consistent with this assignment. Hence, the structure is assumed

to result from both the electronic multiplicity and vibrational coupling.

The magnitude ($\Delta A/A \sim 1 \times 10^{-2}$) and dispersion of the MCD together indicate that paramagnetic C terms predominate¹⁻⁴. These arise from differential population of the sub-levels of a degenerate ground state split by a magnetic field. Such terms are expected for metXbCN (X=M,H), because the ground state is a Kramers doublet. Their calculation requires the ground state g values, which are known from e.p.r. measurements, and electric dipole transition matrix elements. We have made such a calculation using standard MCD theory and a number of reasonable approximations. Following Shulman *et al.*¹⁶ the iron d and porphyrin π molecular orbitals are assumed to approximate their form in D_{4h} symmetry. Using the e.p.r. data and ground state wave functions reported by Shulman *et al.*, we then obtain for transitions to excited states which are both $^2A_{1u}$ and $^2A_{2u}$ in D_{4h} symmetry

$$\frac{\Delta A}{A} \approx + \frac{g_z \beta H}{2kT}$$

where g_z is the component of the ground state g tensor perpendicular to the porphyrin plane, β is the Bohr magneton, H is the magnetic field strength in the direction of the light beam, k is Boltzmann's constant and T is the absolute temperature. With $g_z = 3.4$ (ref. 16), and at 39.2 kG and 25° C equation one predicts $\Delta A/A \approx 1.5 \times 10^{-2}$. The experimental value of $\Delta A/A$ is nearly constant throughout the near IR bands and in good agreement in both sign and magnitude with the predicted value for $^2A_{1u}$ and $^2A_{2u}$ upper states. The MCD data thus strongly support the porphyrin-to-iron charge-transfer assignment. Because the two electronic transitions are predicted to have the same $\Delta A/A$, the MCD does not provide a means of further analysing the structure of the bands.

We have also measured the CD and MCD of the ferric high-spin derivatives metHb and metMb which each exhibit a broad absorption band at $\sim 1,000 \text{ nm}^{18}$. Again the CD is undetectable while the MCD is large. The shape of the MCD is quite different, however, from that in the low spin cyanide derivatives. Thus, not only can the MCD assist in the theoretical analysis of the near IR spectra of haemoproteins, but it can also provide a sensitive and diagnostic indicator of their spin states. The latter should make MCD a very useful analytical tool in the study of haemoproteins.

A more detailed discussion of the work outlined here will be presented elsewhere. We are currently extending our studies of haemoproteins and also investigating applications of IR CD and MCD to other biological systems.

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Halogenated Hydrocarbons in and over the Atlantic

DURING the past few decades the production of the chloro-fluorocarbons, the propellant solvents for aerosol dispensers, has grown exponentially. R. L. McCarthy (unpublished) estimates that the integrated production of CCl_2F_2 and of CCl_3F , the two principal compounds of the class, was about one megaton of each in mid 1971; they are unusually stable chemically and only slightly soluble in water and might therefore persist and accumulate in the atmosphere. Preliminary tests in 1970 showed that CCl_3F was present in the air over Ireland at concentrations exceeding 10^{-11} by volume¹.

The presence of these compounds constitutes no conceivable hazard; indeed the interest lies in their potential usefulness as inert tracers for the study of mass transfer processes in the atmosphere and oceans. The voyage of the RRS Shackleton from the United Kingdom to Antarctica and back in 1971-72 provided an excellent opportunity to compare the observed global distribution of CCl_3F with the predicted distribution from models of the behaviour of an ideal inert gas. Additionally the apparatus used gave simultaneous measurements of two unexpected halomethanes: methyl iodide and carbon tetrachloride. These two compounds and CCl_3F were uniformly distributed both in air and sea.

Analysis was by gas chromatography. A novel electron capture detector capable of coulometric (absolute) measurements was used². Air and seawater samples were taken several times daily and analysed directly; our observations are summarized in Table 1 and Figs. 1 and 2.

Table 1 Detected Levels of Halogenate Hydrocarbons

	CCl_3F	CH_3I	CCl_4
Mean aerial concentration $\times 10^{-12}$	49.6 (7.1)	1.2 (10.0)	71.2 (6.86)
Mean surface water concentration ml. ml. ⁻¹ water (NTP)	7.6 (7.2)	135 (248)	60 (17)
Annual production rate (megatons)	0.44	40	1.7
Residence time (yr)	> 10?	0.003	1

Figures in parentheses are standard deviations.

The release of CO_2 by combustion has been described as an unscheduled geophysical experiment; if so it is a rather undisciplined one, because emissions are small compared with those of the large complex natural cycle. By contrast the emissions of CCl_3F , also unplanned, are much closer to an ideal experiment on a global scale. There are no natural

sources; it is chemically and physically inert; it does not disturb the environment, and can be measured accurately to one part in 10^{12} by volume.

A numerical model of the global atmospheric distribution of an ideal inert gas in space and time was developed by Machta³. In this, the gas was released at an exponentially growing rate with a doubling time of two years in the Northern Hemisphere between latitudes 35° and 55° . The CCl_3F observations are compared with Machta's predictions in Fig. 1 by making the centre point of the model distribution equal to the calculated global mean concentration. The latter comes from the division of the mass integral of CCl_3F production up to the end of 1971 (1.2×10^{12} g) by volume of the atmosphere. The observed and predicted global mean concentrations, 48 and 53 respectively, are in close agreement. That the observed distribution is shallower than the theoretical curve could be due to the difference between the doubling time of the emissions, 3.5 yr and the value of 2 yr used in the model. Except in the north temperate regions the surface waters were not in saturation equilibrium with the air. A few measurements down to 300 m at latitude 29° and 14° N showed a decline in concentration with an e^{-1} depth of 130 m.

Biological methylation is a ubiquitous tendency⁴; nevertheless it was surprising to find a familiar organic chemical reagent distributed throughout the oceans. The large difference between air and sea concentrations (Table 1) suggests the sea to be a source. There was no obvious change of concentration with latitude, although there were large local variations; in a separate investigation one of us (J. E. L.) has found marine algae to be a strong source of CH_3I and it seems probable that the origins of this compound are in the oceans and are biological. The atmospheric residence time of methyl iodide has been estimated by Eggleton and Clough (unpublished work) as 50 h; they assumed photolysis in sunlight to be the sole destructive process. This estimate, taken with the mean aerial concentration of 1.2×10^{-12} , implies an annual production of 40 megatons. We suggest that methyl iodide is the natural carrier of iodine between the seas and the land fulfilling a role for this element similar to that proposed for sulphur by dimethyl sulphide⁵.

The presence of carbon tetrachloride in the air is only slightly more unexpected than is its distribution with latitude

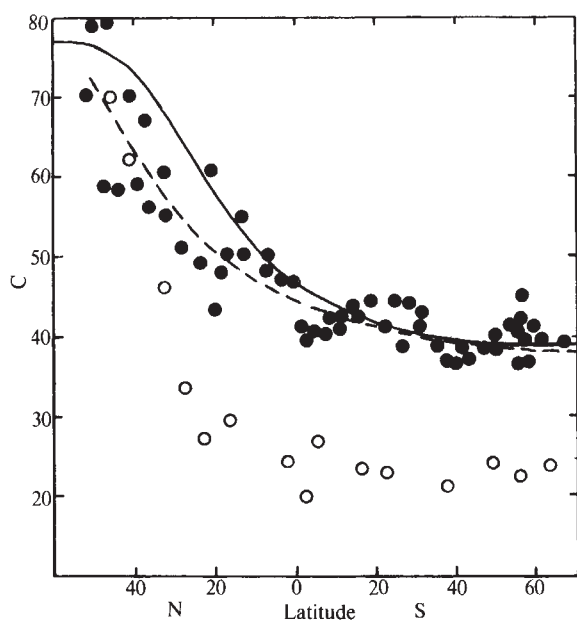


Fig. 1 Distribution of CCl_3F in and over the North and South Atlantic Ocean. ●, Aerial concentrations ($\times 10^{-12}$) by volume. ○, Seawater concentrations ($\times 10^{-12}$); as aerial concentrations in equilibrium with water. —, Theoretical prediction. ---, Best fit third degree polynomial.

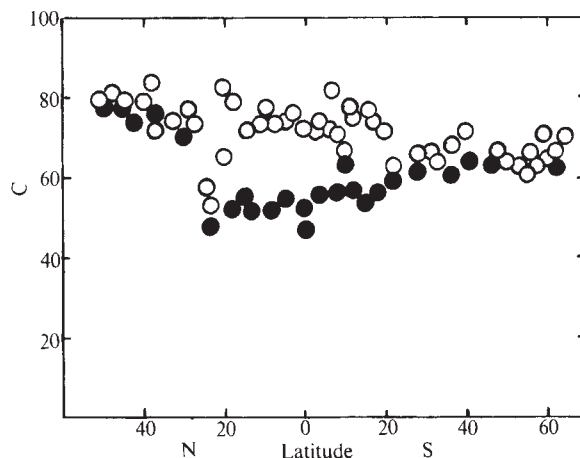


Fig. 2 ●, Aerial distribution of CCl_4 ; ○, marine concentration ($\times 10^{-12}$) by volume. Water concentration ($\times 10^{-12}$) as aerial concentration in equilibrium with water.

shown in Fig. 2. Chlorinated hydrocarbons found in the natural environment are almost always assumed to come from chemical industry, but the aerial concentrations of CCl_4 in the southern tropical regions were as high as those of the northern temperate regions and this is inconsistent with an industrial or domestic source. A few measurements to 300 m showed a decline in CCl_4 concentration with an e^{-1} depth of 50 m. This is a steeper decline than that of CCl_3F , and suggests the hydrolysis of CCl_4 and that the sea is a sink. A single sample of air collected in the stratosphere (provided by D. H. Ehalt) showed CCl_4 to be present there at one fifth the concentration of the upper troposphere; this, taken with the observed concentration minima in the region of the Azores high and the St Helena high, suggests that the stratosphere may also be a sink. The slight fall in aerial concentration seen on going south could be due to some increment from chemical industry but also might be a consequence of the greater area of sea in the south to act as a sink.

The total annual industrial production of CCl_4 is about one megaton of which 95% is used to make CCl_3F and similar compounds. This leaves a maximum of 20 kilotons to escape to the air. Junge⁶ has proposed a method for estimating the approximate residence time of an atmospheric component based on the inverse correlation between residence time and standard deviation of the aerial concentration. From this the residence time of CCl_4 would be about 1 yr, and the annual production rate 1.9 megaton. This is far in excess of any possible industrial source and raises the intriguing problem of the nature of the atmospheric or surface reactions leading to this unlikely product.

All three gases were found in the air and in the sea wherever and whenever they were sought. It is true that the concentrations, less than parts per 10^{10} by volume, were very low and that but for the sensitivity of the analytical technique used they might never have been detected. These low concentrations belie the interest and importance of the three compounds as atmospheric components. They are each an uncomplicated representative of a separate class of compound; CCl_3F is one of the few substances present and easily measured in the air which is wholly anthropogenic in origin; other emissions from the domestic and industrial sector such as the oxides of carbon and sulphur are also components of the natural environment. The unique source and its stability in the air make it an ideal reference substance against which man's other emissions to the environment can be compared. CH_3I is probably a singular product of marine biology and may be a key compound in the natural cycle of iodine between the seas and the land. In spite of its low aerial concentration, the atmospheric throughput is tens of megatons in each year. CCl_4 seems very likely to be a product of natural inorganic chemistry, possibly the reac-

tion of methane in the troposphere with chlorine in a complex sequence of reactions. This substance also is passed through the environment in quantities exceeding 1 megaton annually. In view of the concern over pollution by chlorinated hydrocarbons it is at least consoling to discover that one of them is a natural product.

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Variations in Hydrocarbon Gas Concentration during Supertanker Cleaning Operations

FOLLOWING recent serious explosions aboard very large crude carriers (VLCC), investigations into the possible causes of such accidents have been initiated in a number of countries. Initially, interest was focused on the problems of cleaning and gas freeing of cargo tanks, especially with rotating jet washing machine systems, the electrostatic hazards of which have been reported¹.

Two requirements for ignition of a hydrocarbon gas mixture in a cargo tank are generation of sufficient energy and a tank atmosphere within the flammable range. Before 1960, published information was scarce regarding the explosive conditions which may occur in a tank and the ways in which a tank may be treated during a ballast voyage. In 1960, investigations were reported² that justified a large scale trial of the method of forced ventilation of cargo tanks before and during tank cleaning (the too lean method). Overall results were successful³, and this particular system of tank cleaning has been employed over a number of years aboard Shell ships from 50,000 to 110,000 d.w.t.

However, in December 1969 three VLCCs had serious explosions in one of their centre tanks during tank cleaning. Two of the ships were members of the Shell Group Fleet, M/T Marpassa and M/T Mactra, both 220,000 d.w.t., and the third was the Norwegian ship King Haakon VII, 219,000 d.w.t. While the two Shell tankers had been employing the too lean method for tank cleaning, the Norwegian ship had followed the still quite common practice with no ventilation before or during tank cleaning.

Several of the following investigations⁴⁻⁷ confirmed the earlier results^{2,3}, indicating that during a ballast passage, the tank atmospheres on crude vessels, large or small, during tank washing, or empty on passage, may at any time be within the

flammable range. Over-rich atmospheres were found to be the exception rather than the rule.

Since September 1970 we have been investigating various aspects of explosion hazards aboard VLCCs. Here we discuss some of the results obtained by exact measurements of gas concentrations aboard a VLCC, T/T Tabriz (210,000 d.w.t.), before and after unloading of Kuwait medium crude oil as well as before, during and after tank cleaning operations. Gas samples were drawn through rubber tubes, usually from 10, 40 and 70 foot levels below deck and both in the forward and aft sections of the tanks. The concentrations of each of the hydrocarbon gases from methane to hexane in the gas samples were determined by gas chromatography (GC) using a flame ionization detector (FID) and a 24 foot $\times$ 1/8 inch i.d. copper tubing packed with 20% di-isodecylphthalate on 80-100 mesh 'Chromosorb W' as the chromatographic column. The column readily separates the C_1 - C_6 hydrocarbons which are determined using response curves obtained with pure standards. Hydrocarbon gas compositions are expressed in per cent by volume.

We summarize the essential results from our measurements in one of the wing tanks, port tank No. 1 (14,555 m³). The values reported from the empty tank refer to gas samples drawn from 40 foot below deck in the middle section of the tank.

Fig. 1 shows the variations in the total hydrocarbon gas concentration from the time of unloading until ventilation and washing. The uncleaned empty tank remained in the explosive range during the first days after unloading, and the total hydrocarbon gas concentration increased with time owing to gassing from oil deposits on the walls and in the bottom of the tank. Forced ventilation brought the hydrocarbon gas concentration well below the lower explosion limit (l.e.l.) before the washing operation was started. We made continuous measurements during washing, and our results (Fig. 1, inset) show a marked increase in the total gas concentration when washing was started and the subsequent reduction due to the continued ventilation.

We also found that changes in the composition of the hydrocarbon gas mixture in the ullage and in the empty tank before and during washing were very pronounced (Fig. 2). In the ullage, the two dominating hydrocarbon gases are propane and butane, and both are present in concentrations of approximately 7.5% volume out of a total of approximately 19% volume. When the tank is empty but before ventilation and

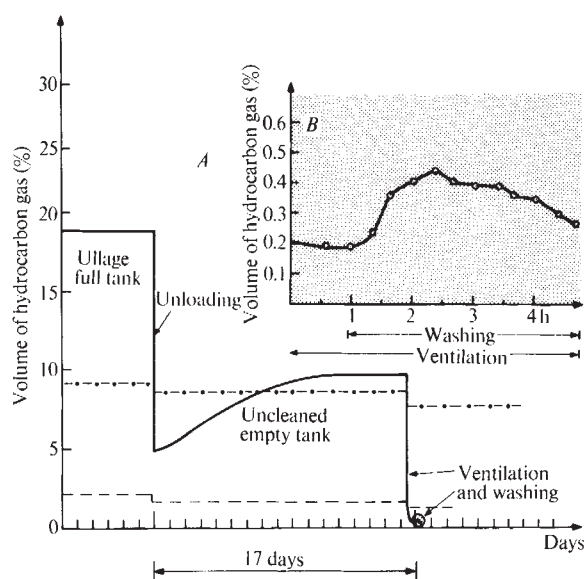


Fig. 1 A, Schematic drawing of the variations with time in the hydrocarbon gas concentration (—), the upper explosion limit u.e.l. (—○—○) and the lower explosion limit l.e.l. (---) in port wing tank No. 1. B, The variations with time (h) in the hydrocarbon gas concentration during the ventilation and washing periods, enlarged.

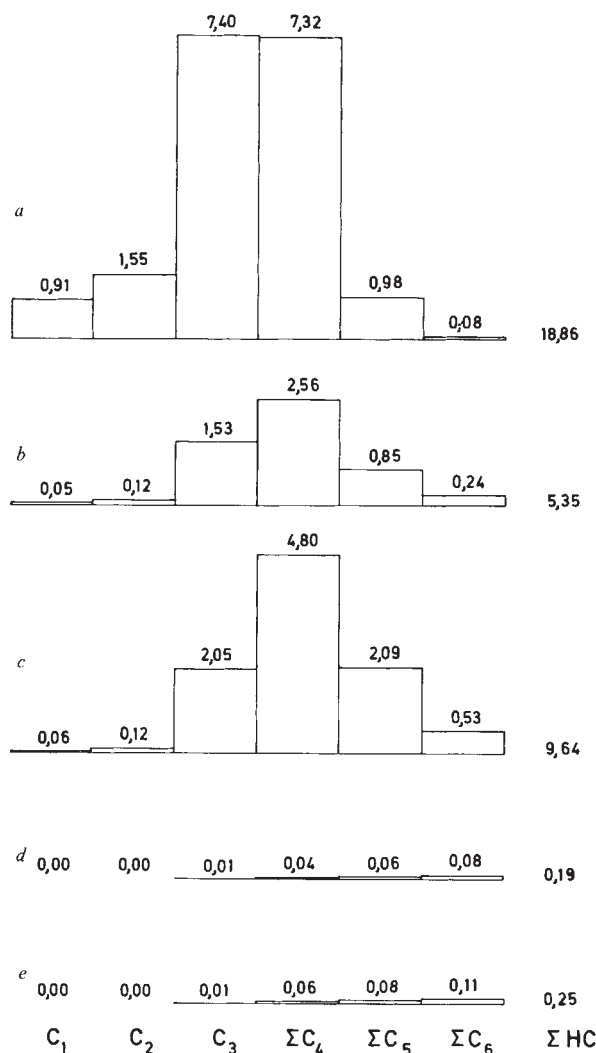


Fig. 2 Typical variations in hydrocarbon gas concentration in the ullage and in the empty tank before and during washing (C₁=methane, C₂=ethane, C₃=propane, ΣC₄=butanes, ΣC₅=pentanes, ΣC₆=hexanes, ΣHC=total hydrocarbon gas concentration, all values in volume % gas). a, Before unloading. b, After unloading. c, Before ventilation. d, Beginning of washing. e, End of washing.

washing, butane is the major constituent followed by propane and pentane, with methane and ethane present in negligible concentrations. By the end of the washing period, the major constituents of the tank atmosphere are hexane and pentane.

These changes in the composition of the hydrocarbon gas mixture also result in variations in the values of the lower (l.e.l.) and the upper explosion limits (u.e.l.) of the mixture in air (Fig. 1). In the ullage, the l.e.l. is 2.09% volume, while the u.e.l. is 9.43% volume. By the end of the washing period, the l.e.l. is 1.36% volume, while the u.e.l. is 7.88% volume.

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Relation of Pleistocene Migrations of Pygmy Stegodonts to Island Arc Tectonics in Eastern Indonesia

PLEISTOCENE pygmy stegodonts have been described from the Indonesian islands of Sulawesi, Flores and Timor¹⁻⁴ which are now separated by deep seas. Hooijer⁴ suggested that the two forms *Stegodon trigonocephalus florensis* and *Stegodon timorensis* apparently coexisted in Flores as well as in Timor, although they may not have been strictly contemporaneous. He concluded that the dwarfing *Stegodon* populations wandered back and forth between Flores and Timor during the Pleistocene, because otherwise it is hard to see how the two evolutionary products on Timor and Flores could have been so similar. Flores is now separated from Timor by a 3,000 m deep Savu Sea 150 km wide which narrows to 30 km in the Ombai Strait between Alor and Timor (Fig. 1). Because an elephant (and presumably a stegodont) could not swim across the Savu Sea and Ombai Strait, the existence of a land connexion between Flores and Timor during the Pleistocene must be postulated (Fig. 1).

The suggestion of a land connexion between northern Timor and the chain of volcanic islands of Flores, Alor and Wetar is supported by the stratigraphy of the Viqueque Formation in northern Timor⁵. The lower members of the Viqueque Formation, which range in age from late Miocene to late Pliocene (equivalent to the planktonic zones N.18–N.21 of Blow's scheme⁶), consist of a basal part deposited in a relatively deep open sea and an upper part deposited in a much shallower sea near the shore. This indicates that the Pliocene sea which covered large parts of Timor became much shallower in the late Pliocene. The older members of the Viqueque Formation are widely distributed in Timor, but the bulk of the formation, which is of Pleistocene age (equivalent to zone N.22 of Blow's scheme⁶), is not developed anywhere in northern Timor (Fig. 2). This large break in the Viqueque succession is a feature of northern Timor where the basal facies of Pliocene age is overlain unconformably by the late Pleistocene and Holocene terraces of reef limestones or alluvium^{5,7,8}. The lower and middle Pleistocene marine deposits absent from northern Timor are extensively developed in other parts of the island^{5,7}. Their absence implies that northern Timor was above sea level during at least part of the early and middle Pleistocene.

Except for the occurrence of the pygmy stegodonts in Flores and Timor there is no available geological evidence to demonstrate that this Pleistocene land area in northern Timor connected with the Inner Banda Arc. It is not necessary to postulate that the entire region between Timor and Flores was land; pygmy stegodont migrations might have taken an indirect route *via* Alor. The Ombai Strait between Alor and Timor offers much the shortest route for

migrations between the Inner and Outer Banda Arcs (Figs. 1 and 2), and the glacial lowering of sea level may have been a critical factor in establishing a land connexion between these regions during the early and middle Pleistocene.

It has been argued⁹ that the late Pliocene-early Pleistocene orogenic phase in Timor resulted from a collision between Australian continental crust (which extended north of Timor) and a subduction zone which lay south of the volcanic island chain of Flores, Alor and Wetar. The absence of recent volcanic activity in Alor, Atauro, Wetar and Romang islands of the Inner Banda Arc, that are closest to Timor¹⁰, may be associated with the apparent cessation of subduction in this part of the Banda Arcs^{11,12}. We have postulated that a period of uplift in this region between the Inner and Outer Banda Arcs occurred directly after the orogenic folding phase in the early Pleistocene, on the basis of evidence from the Viqueque Formation and the distribution of pygmy stegodonts. Such uplift could be expected to follow the cessation of subduction in a zone where all available oceanic crust had been subducted⁹ (Fig. 1).

The presence of the Savu Sea, Ombai and Wetar Straits, now all more than 3,000 m deep, in a region that we suggest was partly land in the middle Pleistocene, implies that late Pleistocene and Holocene downfaulting occurred between the Inner and Outer Banda Arcs. The vertical displacements involved are at least 3,000 m. After the middle Pleistocene Timor and the volcanic island chain of Flores, Alor and Wetar have been uplifted, as indicated by raised terraces, often covered by reefs, up to 765 m in Timor^{5,10}, 250 m in Flores, 700 m in Alor and 820 m in Wetar¹⁰. The deeply incised alluvial terraces of these islands (from which the stegodont bones were obtained^{1,3,4}) also indicate this recent uplift.

Hooijer³ recently concluded that there are two species of *Stegodon* in Sulawesi; a pygmy species *Stegodon sompoensis* very similar to *Stegodon timorensis* of Flores and Timor and the *Stegodon trigonocephalus* similar to the much larger Java species. This discovery of large and pygmy species in Sulawesi raises the possibility of Pleistocene migrations of

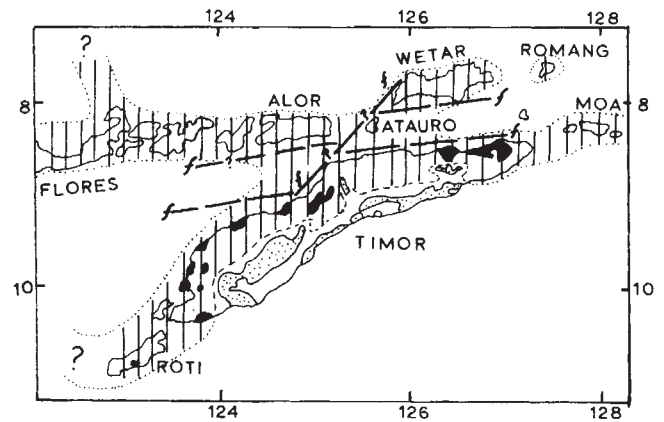


Fig. 2 Map of part of the Lesser Sunda Islands to show (vertical stripes) the early and middle Pleistocene land areas of northern Timor and their postulated connexion with the Inner Banda Arc. The postulated position of recent faults which have dropped the floor of the Savu Sea, Ombai and Wetar Straits by at least 3,000 m are indicated. The outcrop of the important Pleistocene diastem in the Viqueque is shown (in black). The dotted areas represent thick Pleistocene marine Viqueque Formation deposits.

dwarfing populations between Java, Flores and western Sulawesi.

At present south-west Sulawesi is connected to the north-east tip of Flores by a submarine ridge nearly 3,000 m deep that separates the Flores and Banda Seas (Fig. 1). The early and middle Pleistocene uplift, that we suggest affected Timor and the Flores-Wetar region, may have extended northwards to allow the pygmy stegodonts to wander back and forth between Flores and Sulawesi along the Kaloatoa-Matjan ridge, then via Tanahjampea and Salajar ridge. This ridge system may also have been elevated above sea level by a regional uplift that followed the plate collision between eastern and western Sulawesi in the late Pliocene-early Pleistocene⁹. Another land connexion may have developed, as a result of glacial lowering of sea level, across the Sunda Shelf (now 200 m deep) from Java via the Doangdoang shoals to Makassar in western Sulawesi. If these were the stegodont migration routes they imply late Pleistocene and Holocene down-faulting of the Flores Sea and southern Makassar Strait, analogous with the postulated recent down-faulting in the Savu Sea, Ombai and Wetar Straits, which followed the uplift that occurred after continental crust of the Australian plate collided with a subduction zone⁹.

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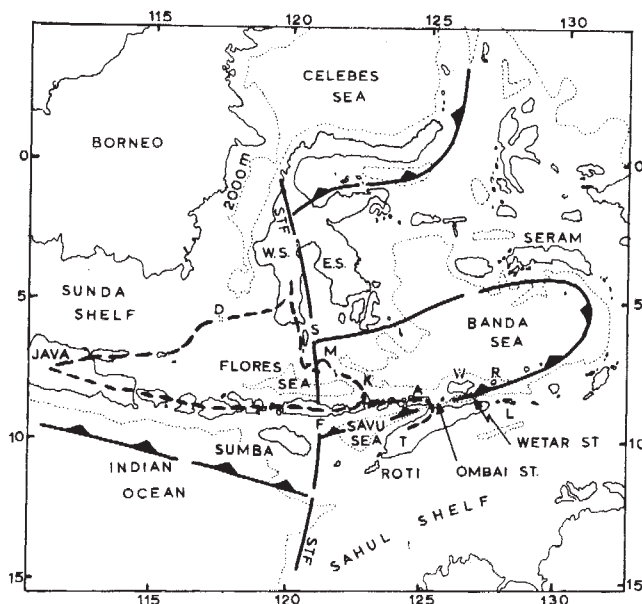


Fig. 1 Map of eastern Indonesia to show postulated Pleistocene migrations of stegodonts. In the Celebes Sea and Makassar Strait the 2,000 m bathymetric contour has been drawn, but elsewhere only the 3,000 m contour is shown. WS, Western Sulawesi; ES, eastern Sulawesi; S, Salajar Is; M, Matjan Is; K, Kaloatoa Is; D, Doangdoang shoals; F, Flores; A, Alor; W, Wetar; R, Romang; L, Leti; T, Timor; STF, Sarasina transform⁹. ---, Stegodont migrations; ▲, early Pleistocene plate margin; ▴, indicates subduction.

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BIOLOGICAL SCIENCES

Geographical Uniformity of Protein Polymorphisms

ELECTROPHORETIC polymorphisms in several species of *Drosophila* in both North and South America and in Europe show considerable uniformity of allele frequencies over a wide geographical range¹⁻³. It has been suggested by Kimura and Ohta⁴ that this finding is compatible with the infinite neutral allele hypothesis of Kimura and Crow⁵ as "it is possible that in animal species such as *Drosophila*, mouse and man well able to migrate, no local population is sufficiently isolated to prevent the entire species or subspecies from forming effectively one panmictic unit". This claim is supported by reference to the theoretical work of Maruyama^{6,7} on the asymptotic rate of decrease of heterozygosity in the absence of mutation, which would seem to be of little relevance to the pattern of variability expected under the joint forces of mutation and genetic drift. Here I show that use of a more appropriate model, which includes the effect of mutation, leads to a quite different conclusion.

Malécot⁸ has made a theoretical study of the coefficients of inbreeding and kinship under a continuous, two-dimensional dispersal model. He implicitly defines the coefficient of inbreeding, f_0 , as the probability that two homologous loci in an individual are identical by descent and that neither of them has undergone mutation since the common ancestor from whom they arose. The coefficient of kinship, f_x , between two individuals at a distance x apart is defined in a similar way. This definition of the coefficients of inbreeding and kinship, which takes mutation into account, circumvents the difficulty encountered in the usual definition, which ignores mutation, that if one traces genes back far enough all genes are from a common ancestor so that every individual is completely inbred. Under the usual definition inbreeding must always be taken relative to a fixed point in the not too far distant past, but under Malécot's definition it is sensible to consider an equilibrium of inbreeding relative to the very distant past. His definition is most easily interpreted under the infinite neutral allele hypothesis since in this case any mutation must be to a new allele not already present in the population. It follows that f_0 can be interpreted as the probability of homozygosity and f_x as the probability that two alleles chosen at random from individuals at a distance x apart are identical in state.

Malécot⁸ has shown that an approximate formula for f_0 is

$$f_0 = [1 + 4\pi\sigma^2\delta/\ln(1/2u)]^{-1} \quad (1)$$

where σ^2 is the mean square dispersal distance between the breeding places of parent and offspring, δ is the density of the breeding population and u is the mutation rate per gene per generation. An approximate formula for f_x , valid for $x > \sigma$, is

$$f_x = \frac{(1-f_0)}{2\pi\sigma^2\delta} K_0(2\sqrt{ux}/\sigma) \quad (2)$$

where K_0 is a modified Bessel function of the second kind. These formulae are valid whatever the shape of the dispersal distribution. If we define $\rho_x = f_x/f_0$ as a measure of the correlation between individuals (or populations) at a distance x apart, it follows that

$$\rho_x = 2K_0(2\sqrt{ux}/\sigma)/\ln(1/2u) \quad (3)$$

This quantity depends only on the mutation rate, u , and on the standardized distance, x/σ , expressed in units of the root mean square dispersal distance. Numerical values of ρ_x are given in Table 1.

These results can be used to test the infinite neutral allele hypothesis from observed data on the geographical uniformity of protein polymorphisms. If $p_{ij}(A)$ is the observed frequency of the j th allele at the i th locus at locality A , the chance that a

gene chosen at random from locality A will be identical in state with a homologous gene chosen at random from locality B can be estimated by $\sum_{i,j} p_{ij}(A)p_{ij}(B)/n$, where n is the number

of loci examined, whereas the chance of homozygosity of an individual chosen at random with equal probability from A or B can be estimated by $\sum_{i,j} (p_{ij}^2(A) + p_{ij}^2(B))/2n$. The ratio of

these two quantities, r_{AB} , is thus an estimate of ρ_x . Values of r have been calculated in this way for the three non-isolated populations of *Drosophila pseudoobscura* studied by Prakash, Lewontin and Hubby¹. Of the 24 loci studied 14 were found to be polymorphic in at least one population, while 10 loci were fixed at the same allele in all populations; r has been calculated using only the 14 polymorphic loci and also using all 24 loci. The results are shown in Table 2. These results cannot be reconciled with the theoretical predictions shown in Table 1 except under the absurd assumption that the root mean square dispersal distance, σ , is of the order of 2,000 km.

Table 1 Theoretical Values of ρ_x

x/σ	u	10^{-4}	10^{-5}	10^{-6}	10^{-7}
1	0.95	0.96	0.96	0.96	0.97
2.5	0.73	0.79	0.79	0.83	0.85
5	0.57	0.66	0.66	0.72	0.76
10	0.41	0.53	0.53	0.61	0.67
25	0.22	0.37	0.37	0.47	0.55
50	0.10	0.24	0.24	0.37	0.46
100	0.03	0.14	0.14	0.27	0.37
250	0.00	0.04	0.04	0.14	0.26
500	0.00	0.02	0.02	0.06	0.17
1,000	0.00	0.00	0.00	0.03	0.10

Another difficulty with the infinite neutral allele hypothesis is that it predicts a degree of heterozygosity in excess of the observed amount. The average heterozygosity found by Prakash *et al.*¹ in the three non-isolated populations considered in Table 2 is 0.123. Equating this figure to $1 - f_0$, with f_0 defined in equation 1 and with $u = 10^{-5}$, shows that $\pi\sigma^2\delta = 0.38$. But $\pi\sigma^2\delta$ is the number of flies within a circle of radius σ , and to make this number less than 1, σ would have to take an absurdly small value, whereas it has been shown in the preceding paragraph that to account for the geographical uniformity σ must take an absurdly large value. A way out of this dilemma would be to suppose that the population had increased fairly recently and not yet reached an equilibrium level of heterozygosity; to explain the geographical uniformity it would also be necessary to suppose that the whole present population had spread from a single centre of dispersion. The fact that a similar amount of heterozygosity with considerable geographic uniformity has been found in different species in different parts of the world, however, makes this explanation unlikely.

Table 2 Estimated Values of r for *Drosophila pseudoobscura*

Pair of populations	Distance (km)	r (polymorphic loci)	r (all loci)
S and M	1,200	0.985	0.992
S and A	2,400	0.981	0.990
M and A	1,250	0.995	0.997

S—Strawberry Canyon, Berkeley, California.

M—Mesa Verde, Colorado.

A—Austin, Texas.

An alternative to the infinite neutral allele hypothesis is to suppose that there is only a small number of neutral alleles at each locus, but that the effective population size is large enough for mutation pressure to maintain considerable heterozygosity at these alleles. To explain geographical uniformity under this model it is necessary to suppose that the effective population size is large enough to make drift negligible, so that the allele frequencies are close to their equilibrium values. If a

locus is considered with two neutral alleles with forward and backward mutation rates μ and ν , the equilibrium gene frequencies of these alleles are $P = \nu/(\mu + \nu)$ and $Q = \mu/(\mu + \nu)$ respectively. Under the same continuous two-dimensional dispersal model as before it has been shown by Malécot⁹ (using a more direct argument not dependent on identity by descent) that the variance in gene frequencies between very distant populations is $f_0 PQ$, and that the correlation in gene frequencies between populations at a distance x apart is ρ_x , where f_0 and ρ_x are as defined in equations 1 and 3 except that $(\mu + \nu)$ must be substituted for u . (It must be remembered that μ and ν are mutation rates per nucleotide rather than per cistron.) If p_A and p_B are gene frequencies in two populations at a distance x apart, it follows that the expected value of $\frac{1}{2}(p_A - p_B)^2$, which may be taken as a measure of the variability of gene frequencies between these two populations, is $f_0(1 - \rho_x)PQ$. In order that this quantity should be small it is necessary for f_0 to be small, say less than ϵ , which implies that $\pi\sigma^2\delta$ is greater than $-(1 - \epsilon)\ln[2(\mu + \nu)]/4\epsilon$; for example, if $(\mu + \nu) = 10^{-7}$ I postulate that $\pi\sigma^2\delta > 382$ to make $f_0 < 0.01$. It does not seem unreasonable to suppose that the number of flies within a circle of radius σ should be of this order of magnitude. The effect of balancing selection can be approximately incorporated into the above model by adding the linearized selection pressure to $(\mu + \nu)$; for example, if the two homozygotes are at selective disadvantages s_1 and s_2 , the quantity $s_1 s_2 / (s_1 + s_2)$ should be added to $(\mu + \nu)^{10}$. Only a very weak selection pressure need be postulated to explain the observed polymorphisms.

I conclude that data on electrophoretic polymorphisms in *Drosophila* are inconsistent with the infinite neutral allele theory of Kimura and Crow⁵, but that they are consistent with the existence of a small number of alleles at each locus which are either neutral or subject to balancing selection. The geographical uniformity of these polymorphisms shows that the effect of genetic drift is negligible, so that all the neutral or heterotic alleles are maintained near their equilibrium gene frequencies.

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Ancient Chromosomal Polymorphism in Hawaiian *Drosophila*

POLYMORPHISM due to inverted chromosome segments is known in natural populations of many living species, including man^{1,2}. A specific inversion arises in one member of a monomorphic chromosome pair and is apparently monophyletic. Where fine-scale mapping of the breaks is possible, as in giant polytene chromosomes, an indelible marker, unique in the history of the species, is produced. Accordingly, in favourable geographical circumstances, inversions can be used as tracers of ancient

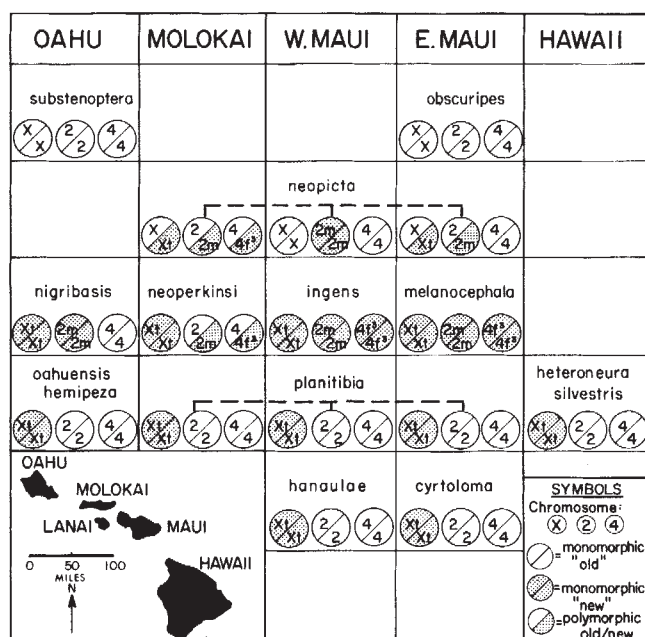


Fig. 1 Geographical distribution and relevant chromosomal conditions in fourteen large *Drosophila* species found on four southeastern Hawaiian islands. For details see text.

populations³. Such precision is not possible with visible mutations or electrophoretically detected variants.

After an inversion has occurred, there exist two alternate arrangements of genes. In the case of a naturally-occurring polymorphism, the ancestral and derived arrangements cannot be recognized by any intrinsic property of the chromosome itself. The "oldness" of one of the alternates must be established in some other manner.

I wish to describe a method of establishing these relationships for particular polymorphisms, based on data from a species endemic to the Hawaiian Islands, *Drosophila neopicta*. Most of the data on which these formulations are based have been published^{4,5}. The giant chromosomes of ninety-six closely related species of Hawaiian *Drosophila* (the "picture-winged" group) have been mapped⁶. The sequential karyotypes of each species and its polymorphisms have been described in terms of an arbitrary standard (*Drosophila grimshawi*). Among these species, 198 inversions have been found. Many of these (123) have never been found in the polymorphic state within any species; they are recognized by inspection of the banding order in the homozygous state. Because of the exceptional quality of the chromosomes of this group, recognition of the interspecific homology of all banded regions of each of the five major polytene chromosomes is possible.

An important feature of these data is that it is possible to determine, by direct cytological examination, whether a species has, or does not have, a particular banding sequence in one or more sections of its karyotype. I shall focus attention on three of the eight polymorphisms known in populations of *D. neopicta*. These are designated X/Xt , $2/2m$ and $4/4f^3$; each difference is due to a single inversion in chromosomes X , 2 and 4 respectively. The geographical distribution of this species and of the above three pairs of alternate gene arrangements is given in Fig. 1 (upper centre). The samples are small (6 to 18 chromosomes from each of the three volcanoes) so that the absence of polymorphism in several instances may not be significant.

The question arises: is it possible to recognize any of these alternate gene arrangements as derived? Evidence that Xt , $2m$ and $4f^3$ are derived exists (Table 1). Xt is fixed in eleven species but is lacking in eighty-four; this statement is based on a large number of chromosome examinations^{5,6}.

The standard X segment is found in a diversity of species belonging to five subgroups of the picture-winged *Drosophila*.

Table 1 Three Chromosomal Polymorphisms found in *D. neopicta* and Distribution of the two Gene Arrangements Comprising each Polymorphism (X/Xt ; $2/2m$; $4/4f^3$) in Ninety-five Related Hawaiian Species

Species or chromosomal subgroup		No. of species	N_A	N_X	Gene arrangement present (+) or absent (-)					
					X	Xt	2	2m	4	4f ³
(I)	Grimshawi	56	2,765	2,075	+	-	+	-	+	-
(III)	Adiastola	14	422	330	+	-	+	-	+	-
(IV)	Punalua	8	392	303	+	-	+	-	+	-
(V)	Primaeva	2	94	68	+	-	+	-	+	-
(II)	Planitibia (in part: <i>D. picticornis</i> , <i>D. setosifrons</i> , <i>D. substenoptera</i> , <i>D. obscuripes</i>)	4	186	142	+	-	+	-	+	-
	<i>D. neopicta</i>	1	36	29	+	+	+	+	+	+
	<i>D. neoperkinsi</i>	1	12	10	-	+	+	+	+	+
	<i>D. nigribasis</i>	1	16	12	-	+	-	+	+	-
	<i>D. ingens</i> and <i>D. melanocephala</i>	2	18	13	-	+	-	+	-	+
	Remainder of (II) Planitibia subgroup (see Fig. 1)	7	180	144	-	+	+	-	+	-
Total		96								

N_A , N_X , No. of autosomes and X chromosomes observed.

Among these are the two species of the *primaeva* subgroup. These latter are primitive and are found on the geologically old island of Kauai. They are intermediate in phylogenetic position between the picture-winged species of the newer Hawaiian islands and certain Palearctic and Nearctic continental forms, notably the *robusta* group⁷⁻⁹.

In contrast, the Xt inversion is found only among twelve closely related species of the *planitibia* subgroup, most of which are endemic to the geologically recent (<1.5 m.y.¹⁰) islands of Molokai, Maui and Hawaii (Fig. 1). The subgroup has sixteen members, fourteen of which are unusual in *Drosophila* in that they have an extra cross-vein in cell R-5 of the wing. These features and the distribution of the species suggest their recently-evolved, terminal position in the general phylogeny of the picture-winged species⁵.

The facts concerning the two autosomal polymorphisms supplement those for the X chromosome. Thus, the same eighty-four species which lack Xt also lack arrangements $2m$ and $4f^3$. Accordingly, it may be concluded that standard segments X , 2 and 4 are ancestral and that Xt , $2m$ and $4f^3$ are derived. In Fig. 1, the circular symbols under the species' names reflect this: they are shaded if the gene arrangement concerned is derived and left open if it is ancestral.

Of the fourteen species considered in Fig. 1, only *D. substenoptera* (Oahu) and *D. obscuripes* (East Maui) are monomorphic for all three ancestral chromosomal types (top row; open circles). On the other hand, one or more new inversions are found in the monomorphic state in eleven species (lower three rows; shaded circles, Fig. 1). Each of these latter species must have had, as an ancestor, a population that was at first monomorphic for old chromosomes. Later, the ancestor must have become polymorphic "old/new". Accordingly, the facts suggest that these eleven species are newer than those which are polymorphic old/new. It is not possible to designate any living species as the most primitive in this series. This is because of the possibility of refixation, in a modern species, of an old chromosome from an old/new polymorphism in its ancestor.

Conditions in *D. neopicta* are especially instructive. Populations from Molokai have old/new polymorphisms in three chromosome pairs. *D. neoperkinsi*, a Molokai endemic sympatric with *neopicta*, has the same two autosomal polymorphisms. Only these two species carry old/new polymorphisms; the nine others having new arrangements carry them in the homozygous state.

The possibility needs to be considered that the old/new polymorphisms are due to recent hybridization rather than ancient polymorphism. The strongest evidence against this hypothesis is the fact that, among all the species of the *planitibia* subgroup on Molokai and Maui, *neopicta* is extraordinarily disparate in size, morphology and especially in mating behaviour (ref. 11 and unpublished results of H. Spieth). Thus,

in contrast to the others, it is a small fly with poorly developed lek behaviour. On these grounds, matings may be considered unlikely. Each of the species is highly distinctive and each retains these characteristics in laboratory cultures.

The facts strongly suggest that all twelve existing species which carry new arrangements were ultimately derived from a singly, doubly or triply polymorphic ancestral population. The latter condition exists today in the *neopicta* population of Molokai. The proposed ancestral population was probably also a large one. Whereas most of the species under consideration here are geographically confined and chromosomally monomorphic, *neopicta* is widespread, suggesting that similar populations could have existed in the past. Also, if the putative ancestral population was continuously small, confined, and only lately expanded, it would seem unlikely that it could retain the three ancient polymorphisms over the geological time indicated. On the contrary, random drift would be expected to eliminate this variability.

Large, geographically widespread and chromosomally polymorphic populations seem to be unusual in the Hawaiian fauna. More than 70% of the picture-winged species are without any intraspecific chromosomal polymorphism; most have restricted distributions and seem to exist in small populations⁵.

Genetic features of past populations have been inferred from conditions in descendant species by several authors^{9,12,13}. The following view is consistent with these ideas. I suggest that certain large, highly heterozygous, long standing populations may be especially prone to produce small, semi-isolated, inbred populations on their margins. These latter may become species through peripheral isolation and the founder effect^{3,14}. If the large population can survive over a long period it may remain competent for speciation and may continue to bud off new species while retaining its ancestral character. Only a few of the new species formed might acquire the wide distribution, large size and heterozygosity of their ancestor. The rest of the species, with small, geographically confined populations would be expected to be less competent for speciation.

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Hypotaaurine: Regulation of Production in Seminal Vesicles and Prostate of Guinea-pig by Testosterone

THE accessory sex organs of the male are dependent on androgens for their growth, maintenance and secretory activity. These physiological effects are regulated through the RNA-protein biosynthesis system in the various animal species. The protein of the guinea-pig seminal vesicles, seminal fluid and prostates decreases after castration and increases after testosterone administration in direct proportion to the changes in tissue weights without change in amino-acid composition. The rate of incorporation into protein of 2-¹⁴C glycine *in vivo*² and several ¹⁴C amino-acids by cell free preparations of these tissues³ is decreased by castration and increased by testosterone with concomitant changes in RNA⁴.

As protein synthesis is dependent on the availability of the necessary amino-acids, the soluble fractions of these tissues were analysed for their respective amino-acids and other ninhydrin positive substances. The striking changes in the concentration of hypotaaurine after castration and testosterone administration are presented here because of the implications with respect to fertility and other physiological and pathological changes in the prostate and seminal vesicles.

The guinea-pigs were castrated at approximately 3 months of age. Three weeks later two pellets of testosterone (approximately 15 mg each) were implanted subcutaneously^{3,4} and the animals were killed after 23 day. The prostates and seminal vesicles were separated, cleaned of extraneous tissue and the seminal fluid removed from the seminal vesicles. The weighed organs were wrapped separately in aluminium foil, placed in a sealed plastic bag and frozen at -20° C. The coagulated

seminal fluid was extracted immediately. The free amino-acids were extracted with 6 M HClO₄ essentially as described by Saifer⁵. Norleucine⁶ was added at the beginning of extraction as an internal standard. The analyses were performed on a 'Beckman/Spinco Analyser, Model 120B'.

The concentration of hypotaaurine and taurine in the seminal vesicles and prostate was much greater than that of the sulphur amino-acids (Table 1) and also of the common amino-acids and other ninhydrin-reacting compounds (unpublished). The hypotaaurine in both tissues was drastically reduced by castration and restored by testosterone treatment. The concentration of taurine was not significantly altered by either treatment in the seminal vesicles and was slightly altered in the prostate while half-cystine and methionine were increased by castration and restored to or towards normal by the androgen except for a questionable effect in the cystine of the prostate. The hypotaaurine in the normal seminal fluid was even greater (26.5 μmol g⁻¹) than in the tissue. Taurine, half-cystine and methionine, however, were approximately the same as in the tissue.

Hypotaaurine is derived mainly from cysteine and is an intermediate in the production of taurine in the liver^{7,8}. It is also produced in the fallopian tube of many animal species⁹, ovaries of the ewe⁹ and the sperm¹⁰, and the fluid of the seminal vesicles and epididymis¹¹ of the boar. Production in the fallopian tube of the ewe increases during the latter part of the oestrous cycle when fertilization occurs, and decreases as the concentration of progesterone increases¹¹. The administration of an anti-fertility agent, 6α-methyl-17α acetoxypregesterone, also greatly inhibits hypotaaurine production¹².

Van der Horst and Brand⁹ suggest that the decreased fertility after administration of the anti-steroid is due to the decreased production of hypotaaurine by the fallopian tube and attribute the beneficial effect of hypotaaurine to its strong anti-oxidant property. Our data support this, suggesting that the presence of a high concentration of hypotaaurine in the seminal fluid is important for the maintenance of the spermatozoa and secondarily supplements the production of hypotaaurine by the fallopian tube to provide a favourable environment for fertilization of the ova.

The decrease in hypotaaurine production after castration suggests either a decreased supply of cysteine or else a decrease in the activity and/or production of the enzyme for conversion of cysteine to hypotaaurine. The latter seems to be the more likely possibility since both half-cystine (questionable in prostate) and methionine in the tissue pools were increased after castration and restored to or towards normal by testosterone.

In contrast to the accessory sex organs, the mouse kidney, a tissue highly responsive to androgens, contained only trace

Table 1 Effect of Testosterone on Hypotaaurine, Taurine, Half-Cystine and Methionine Concentration in the Seminal Vesicle and Prostate of the Guinea-pig

Amino-acid	Normal (N) μmol g ⁻¹ ± s.e.	Castrate (C) μmol g ⁻¹ ± s.e.	%	Testosterone (T) μmol g ⁻¹ ± s.e.	%	N vs C P
Seminal vesicles						
Hypotaaurine	6.30 ± 0.68	1.77 ± 0.11	- 72	5.73 ± 0.33	- 9	0.01
Taurine	5.98 ± 0.30	6.49 ± 0.26	+ 8	5.85 ± 0.46	- 2	0.3-0.2
1/2 Cystine	0.04 ± 0.01	0.12 ± 0.01	+ 236	0.07 ± 0.02	+ 86	0.01
Methionine	0.26 ± 0.02	0.48 ± 0.02	+ 88	0.27 ± 0.02	+ 4	0.01
Prostates						
Hypotaaurine	4.63 ± 0.41	0.71 ± 0.08	- 85	3.48 ± 0.33	- 25	0.01
Taurine	4.64 ± 0.39	5.64 ± 0.13	+ 22	4.32 ± 0.23	- 7	0.05-0.02
1/2 Cystine	0.27 ± 0.07	0.38 ± 0.06	+ 38	0.43 ± 0.05	+ 58	0.3-0.2
Methionine	0.37 ± 0.04	0.70 ± 0.02	+ 90	0.39 ± 0.04	+ 5	0.01

There were 10 normal, 28 castrated and 20 testosterone treated guinea-pigs. The mean weights of the seminal vesicles were: N=762, C=241, T=754 mg and of the prostates were: N=585, C=258, T=508 mg. Each amino-acid analysis represents 2 normal, 4 castrated and 2 testosterone treated animals except that one of the treated samples was the pool from 10 guinea-pigs. The values represent the means of 5, 7 and 6 analyses for the respective groups.

quantities of hypotaurine but a greater concentration, two to three times that of the guinea-pig tissue, of taurine which was increased after castration and restored to normal by androgen treatment (unpublished).

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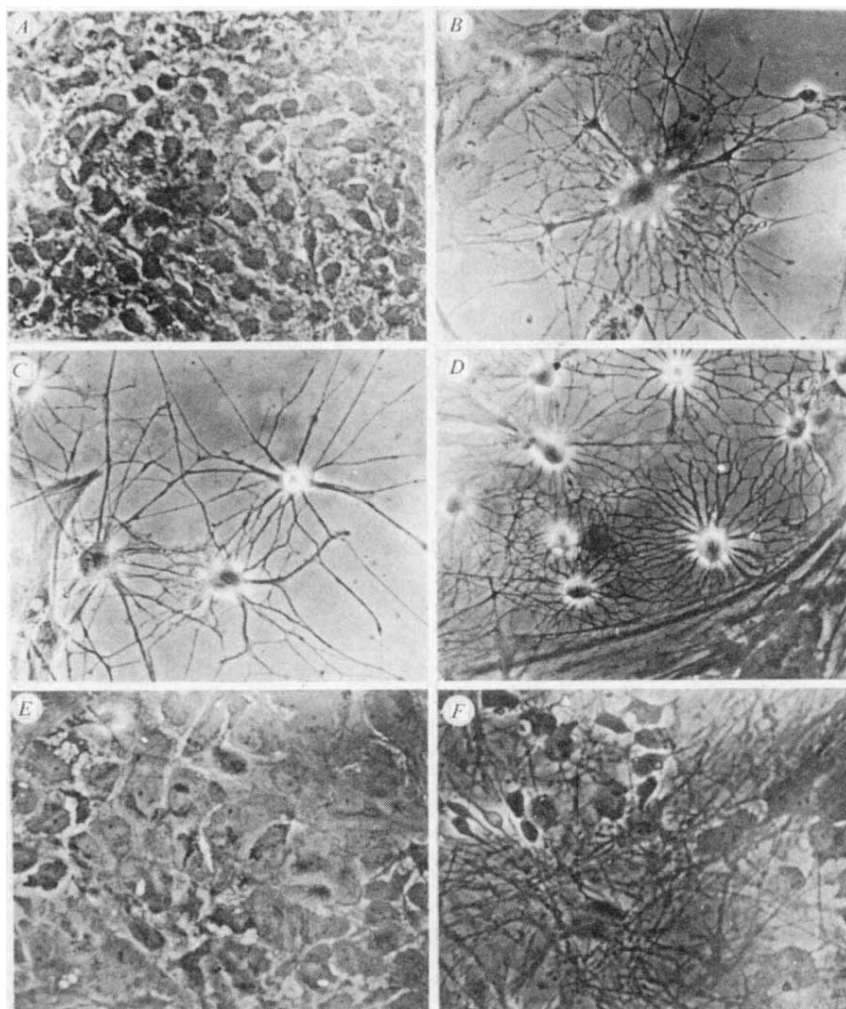
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Morphological and Biochemical Alterations in Foetal Rat Brain Cells cultured in the Presence of Monobutyl Cyclic AMP

I HAVE found that several aspects of the morphology and biochemistry of cultured foetal rat brain cells can be altered by N-monobutyl-cyclic 3',5' monophosphate (N-but cyclic AMP). Dispersed cell cultures of foetal rat brain were grown for 15 days in the presence of 10^{-3} M N-but cyclic AMP and compared with similar untreated cultures. N-but cyclic AMP, prepared from dibutyl cyclic AMP (Sigma) as previously described¹, was used because of its expected greater activity than cyclic AMP in an intact cell preparation². It is more stable than the dibutyl form which may release appreciable quantities of butyric acid¹.

Within 72 h after the addition of N-but cyclic AMP to cultures, cell division (as measured by protein accumulation) slowed, and the morphology of many cells had changed markedly. Multiple cell processes were elaborated, sometimes in a network (Fig. 1A-D). These cells comprised 25-50% of discernible cellular elements after 18 days in culture. They stained poorly with silver (Bodian technique) and were not electrically excitable when stimulated with intracellular electrodes. When N-but cyclic AMP was added to confluent cultures (10 days after plating), processes could still be formed (Fig. 1E and F). The effect on process formation could not be reproduced by inhibiting cell divi-

Fig. 1 Whole brains were obtained from foetuses of 18-21 day pregnant Fisher strain rats (NIH), dissociated in isotonic solution D³ into single cells by passage through 210 μ m and then 130 μ m nylon cloth (Nitex; Tabler, Ernst, and Traber, Elmstord, New York), inoculated into polystyrene culture dishes (Falcon) at a density of 10^5 cells/cm², and grown in Dulbecco's modified Eagle's medium (GIBCO) containing 10% (v/v) foetal bovine serum (Colorado Serum Co., Denver), 10 U/ml. Na-penicillin G, and 10 μ g/ml. streptomycin sulphate. Cultures were incubated at 37° C, 100% humidity, in an atmosphere of 10% CO₂-90% air. Medium (8 ml./dish) was changed on day 3 after plating and on alternate days thereafter; N-but cyclic AMP was first added on day 3 and subsequently at each medium change. The pictures are phase contrast micrographs ($\times 135$). A, Normal culture, 10 days; B-D, cultures treated with N-but cyclic AMP beginning day 3, 10 days; E, normal culture, 14 days; F, culture treated with N-but cyclic AMP beginning on day 10, 14 days.



sion with 5-fluorodeoxyuridine. Cultures treated with N-but cyclic AMP never achieved confluence and differed markedly from control cultures in their amount of protein (Table 1). This limitation of cell division was not accompanied by microscopic changes indicative of cell injury or death.

There were also substantial differences in the activities of the neurotransmitter-related enzymes acetylcholinesterase (acetylcholine acetylhydrolase; EC3117) and choline acetyltransferase (EC2136) (Table 1). In cultures treated with N-but cyclic AMP, acetylcholinesterase specific activity was 300% of the control value; choline acetyltransferase was 48% of the control value. These ratios were achieved by the twelfth day of culture and remained stable until day 18. Cyclic AMP has previously been reported to increase acetylcholinesterase in cultured neuroblastoma cells⁷, and to increase both acetylcholinesterase and choline acetyltransferase in cultures of foetal chick brain⁸.

Table 1 Protein and Enzyme Activities after 18 Days in Culture

	Control	+ N-but cyclic AMP
Protein (mg)/culture	1.890	0.392
Choline acetyltransferase, pmol *	83	40
Acetylcholinesterase, nmol *	2.15	6.45

* Product formed/min/mg protein.

Methods for preparation of cells for assay⁴ and determination of acetylcholinesterase³, choline acetyltransferase³, and protein⁵ have been previously reported. Acetylcholinesterase specificity was verified with Burroughs Wellcome compound 284C51 (1,5-bis-(allyldimethyl-ammoniumphenyl)pentane-1,3-dibromide), 10^{-5} M (ref. 6). The radioactive product of the choline acetyltransferase assay was determined to be >95% acetylcholine by paper chromatography as previously described⁴.

The identity of the process-forming cells could not be determined with certainty. The lack of increase in the activity of choline acetyltransferase, a putative neuronal enzyme, and the electrophysiological and histochemical evidence indicated that they were probably not neurones. Besides neurones and glial cells, a few endothelial, ependymal, and fibroblast-like cells were probably present in the cultures, but the abundance and appearance of the cells with processes suggested that they were of glial origin.

Cyclic AMP (or one of its analogues) can affect cell division^{9,10} and morphology^{2,11} in tumour cells and permanent cell lines. My results show that it markedly affects these properties in primary cultures of cells from the developing rat brain. Enzyme activities were also affected, but it is not known whether the changes resulted from selective growth effects or alterations in gene expression. Mechanisms mediated by cyclic AMP may play a role in mammalian central nervous system development.

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Relationship between Protein Synthesis and RNA Content in Skeletal Muscle

EXTENDING our studies on the adaptation of muscle synthesis to dietary stress^{1,2}, we investigate here the relationship between concentrations of tissue RNA and rates of protein synthesis. Dietary regimes which depress protein synthesis also lead to a fall in tissue RNA content^{3,4}. This fall may reflect a reduced need for cellular RNA components, or it may itself be of primary importance in determining the rate of protein synthesis. More than 80% of muscle RNA is ribosomal and this proportion appears to be maintained during protein depletion⁵, so that a change in RNA also reflects a change in ribosome content. If alteration in ribosomal content effects control, then this alteration may be termed a change in the ribosomal capacity for protein synthesis. If, however, a change in synthesis is brought about by alterations in the other factors modulating each phase of translation, then it is a change in the ribosomal efficiency. This will be demonstrable as alterations in the amount of protein synthesized per unit RNA. We have investigated the extent to which alterations in rates of protein synthesis result from changes in the capacity for and the efficiency of protein synthesis in muscle.

We have measured the rate of muscle protein synthesis *in vivo* by means of the constant intravenous infusion technique⁷. Rats were infused *via* the tail vein with trace amounts of ¹⁴C tyrosine for 6 h; the plasma and intracellular specific radioactivities in tyrosine quickly rise to a plateau, and the rate of protein synthesis was calculated from a single measurement of the specific radioactivity of the free and protein-bound tyrosine at the end of the infusion⁸. Measurements of the rate of protein synthesis and muscle RNA content were made on rats which had been starved, or fed either a standard protein (net dietary protein energy ratio 10%) or a protein free diet. Groups of animals were also fed the 10% diet after 3 weeks of the protein free regime.

Table 1 shows that muscle synthetic rates fell markedly on the dietary regimes which involved starvation or a protein free diet. Refeeding with the 10% diet, after 21 days lacking protein, led to a prompt increase in the rate of protein synthesis but the rates were still less than those in the control group.

Starvation or protein free feeding led to a decline in the RNA concentration, expressed as the RNA to protein ratio, and this was partially restored by refeeding the 10% diet. The rate of protein synthesis per unit RNA (the efficiency of protein synthesis) was calculated from the data in the first two columns of Table 1. Despite considerable variations in the rates of synthesis the animals could be separated into two groups. Group A, with an average efficiency of 14.0 µg protein synthesized/µg RNA day⁻¹, included all the rats receiving protein in their diet. The remaining animals, Group B, were starved or fed the protein free diet and displayed a reduced efficiency with an average of 9.3 µg protein synthesized/µg RNA/day.

The relationship between the rate of synthesis and the RNA concentration for the individual animals in each of the two groups is shown graphically in Fig. 1. Wide variation within the two groups in RNA concentration is correlated with similar changes in the rate of protein synthesis. These variations therefore reflect changes in the capacity for protein synthesis, whereas a transition between the two groups reflects a change in the efficiency of synthesis.

When the time course of the changes is considered, it is clear

from Table 1 that the short term responses of muscle protein synthesis to dietary changes predominantly reflect changes in efficiency of synthesis. The change to a new level of efficiency occurred within 24 h of fasting or feeding the protein free diet, and this change was readily reversible when dietary protein was restored. Such abrupt changes in efficiency as those found with our dietary regimes might not have been so large if graded reductions in the protein content of the diet had been tested. The subsequent changes in protein synthesis were a result of alterations in the capacity, which fell progressively throughout the first 10 days on the protein free regime and then gradually increased with refeeding. The changes in capacity were not as rapid as those in efficiency, but were of equal quantitative importance by the second day of protein free diet and after 10 days were responsible for the major part of the reduction in muscle protein synthesis.

These investigations are similar in some respects to those reported by Henshaw *et al.*⁹, who measured synthesis rates *in vivo* in small numbers of fasting and fed rats by a single injection of large amounts of ¹⁴C lysine. The efficiency of synthesis in their fasted rats fell to between one half and one quarter of the efficiency in their fed animals, but within the fed group their measurements of efficiency varied from values equivalent to 2.2 to 7.7 $\mu\text{g protein}/\mu\text{g RNA/day}$. Because of this variability and the higher efficiencies in our animals no strict comparison between results is possible. Large variations in rates of protein synthesis may occur over 20 min intervals, as measured by Henshaw *et al.*, but these fluctuations would be of little quantitative importance in determining muscle protein mass unless the synthetic rates were sustained for sufficiently long to become apparent with a 6 h infusion. The synthetic rates presented here agree closely with measurements using tracer infusions of glycine¹⁰ and lysine⁶ and with those values calculated from the decay of ¹⁴C carboxyl labelled aspartate and glutamate in muscle protein¹¹. This latter technique makes no assumption about the specific activity of the precursor for protein synthesis.

Most of the information about the control of protein synthesis comes from *in vitro* experiments, many of them involving purified or semi-purified components. It is very difficult to extrapolate directly from measurements *in vitro* to the condition *in vivo*. For example most *in vitro* studies of muscle protein synthesis involve systems in which no initiation occurs, limiting the study to mechanisms of elongation and

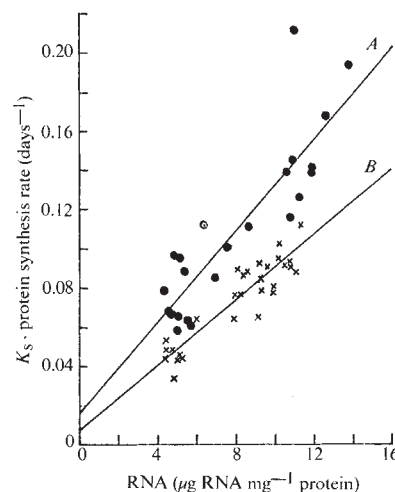


Fig. 1 Relationship between rates of protein synthesis and RNA concentrations in rat skeletal muscle. Results are the individual results shown as means of groups in Table 1. The regression analyses were performed separately on groups A and B. The lines correspond to equations

$$K_s = 0.0117 \frac{\text{RNA}}{\text{protein}} + 0.016 \text{ (group A) } r=0.874$$

$$\text{and } K_s = 0.0084 \frac{\text{RNA}}{\text{protein}} + 0.0070 \text{ (group B) } r=0.909$$

The gradients of the regression lines are significantly different at the level $0.05 > P > 0.02$.

termination^{12,13}, and precluding the quantitative assessment of the importance of any single factor in the intact process *in vivo*. Lack of initiation is one reason why rates of protein synthesis *in vitro* are often much slower than *in vivo*⁹. Our studies demonstrate that while changes in the efficiency of protein synthesis do occur *in vivo* and are important in initiating short term changes in protein synthesis, the changes produced by a longer exposure to dietary stress seem to result from an alteration in the capacity for protein synthesis; that is a loss of the RNA components of the translational apparatus.

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Table 1 Effect of Diet on the Rate of Muscle Protein Synthesis and RNA Content of Rat Skeletal Muscle

Diet	n	Synthesis rate (ks day ⁻¹)	RNA (μg RNA/mg protein)	Synthesis/ RNA (μg protein/μg RNA/day)
Stock				
Control	9	0.153 (0.031)	11.53 (1.01)	13.1 (2.5)
1 day refed	6	0.070 (0.013)	4.91 (0.57)	14.4 (3.0)
2 days refed	4	0.080 (0.020)	5.44 (0.56)	15.5 (4.0)
3 days refed	4	0.095 (0.020)	7.35 (0.88)	14.0 (2.5)
A	23			14.0 (2.86) ¹
Protein free				
1 day	6	0.098 (0.011)	10.50 (0.57)	9.3 (0.8)
2 days	5	0.083 (0.005)	8.37 (0.53)	9.8 (0.6)
10 days	4	0.052 (0.009)	4.78 (0.67)	10.3 (1.6)
21 days	5	0.043 (0.006)	4.78 (0.23)	9.1 (1.5)
Fasted				
1 day	6	0.087 (0.007)	9.59 (1.01)	9.2 (1.3)
2 day	3	0.070 (0.009)	8.71 (0.79)	7.8 (0.5)
B	29			9.3 (1.24) ¹

Significance between numbered groups 1 = $P < 0.001$.

100 g male albino rats were infused via the tail vein with ¹⁴C tyrosine (s.a. 10 mCi mmol⁻¹) in normal saline (2 μCi ml⁻¹) at a rate of 0.5 ml h⁻¹ for 6 h. The rats were then decapitated and the quadriceps and gastrocnemius muscles excised and homogenized in ice-cold 10% TCA. Specific radioactivity of free and protein-bound tyrosine was measured and the rate of protein synthesis calculated⁸. The RNA and protein content were measured¹⁴. Results are shown as mean ± s.d.

Basilar Membrane Correlates of the Combination Tone $2f_1-f_2$

GOLDSTEIN¹ has shown that when two tones of frequency f_1 and f_2 ($f_2 > f_1$) are heard simultaneously, the most prominent distortion product is the combination tone of frequency $2f_1-f_2$. It is observed most strongly when the ratio $f_2/f_1 = 1.1$, when it is typically only 15 dB below the level of the primary tones. The relative amplitude can be measured by cancellation and is substantially independent of input level, that is, it can be considered as an essential non-linearity, but it decreases rapidly as the ratio f_2/f_1 is increased. These features seem to rule out Helmholtz's² explanation based on simple limiting distortion in the middle ear.

Goldstein and Kiang³ extended the psychophysical findings by studying single auditory nerve fibres in cat. They found that under suitable conditions fibres would respond to f_1 and f_2 together but to neither alone. Also, fibres of low characteristic frequency responded in a phase locked manner to $2f_1-f_2$ and could be desynchronized by adding a real frequency component at $2f_1-f_2$ of appropriate amplitude and phase.

These results suggested that the combination tone $2f_1-f_2$ was generated within the cochlea and might be present as a vibration of the basilar membrane. The necessity for two frequency filters, one before the essential nonlinearity and the other following it, was appreciated by both Goldstein^{1,4} and Smoorenburg⁵. The essential nonlinearity was either in the hydrodynamics of the cochlea⁶ or in the hair cell motion. It was assumed that the distortion products were coupled back to the basilar membrane and analysed at their proper place by the second filter. Here we tested some of these assumptions by measuring the mechanical vibration component at $2f_1-f_2$ on the basilar membrane in guinea-pig.

Measurements of basilar membrane vibration were made with a miniature capacitive probe with a tip diameter of 0.15 mm (J. P. W., manuscript in preparation), utilizing the same principle as Békésy⁷. Amplitudes down to 1 Å were measured using a phase sensitive amplifier. The probe was attached to a moving armature system which was used both for the fine adjustments of the probe-membrane distance and also for calibration of the system by vibration of the probe through a known distance.

The guinea-pigs were 300–500 g, and were anaesthetized with pentobarbitone sodium and fixed in a stereotaxic frame with hollow ear bars. They were paralysed with gallamine triethiodide and artificially respired. The cochlea was exposed by opening the bulla, the first turn opened and the perilymph drained from scala tympani. The probe was placed about 20 μm from the surface of the basilar membrane.

Sound was presented closed field from a 'Bruel and Kjaer 4134 microphone' and calibrated with a 1 mm probe tube microphone. The frequency response of the basilar membrane was first measured and plotted automatically from 20 Hz to 20 kHz using the capacitive probe and a 'Bruel and Kjaer 1024' oscillator, '2020 Heterodyne Slave Filter' (3.16 Hz bandwidth) and '2305 Level Recorder'. A 'Brookdeal 401 Lock-In Amplifier' was used for measurements up to 50 kHz and for combination tone measurements. When the frequency giving maximal vibration for a constant SPL input had been established, frequencies f_1 and f_2 were chosen so that $2f_1-f_2$, f_1 and f_2 were in this region and so that the ratio f_2/f_1 was close to 1.1. The vibration at $2f_1-f_2$ was determined from the in-phase and quadrature components on the lock-in amplifier. The synchronizing signal required for the amplifier was generated by a diode mixing circuit and filtered using the 'Bruel and Kjaer 2020 filter'. Care was taken to avoid drift of phase during the measurements.

Threshold for N_1 response to clicks was measured regularly on the bony cochlea and increased systematically throughout the experiment. Opening the first turn and draining the perilymph had no direct effect on threshold, which increased by less than 3 dB.

The frequency response and peak amplitude of the basilar membrane vibration were found⁸ to be similar to those reported by Johnstone, Taylor and Boyle⁹. Although the levels of vibration at $2f_1-f_2$ were measured absolutely they were converted to equivalent levels of sound input and expressed relative to the average level of the primaries (Table 1).

Table 1 $2f_1-f_2$ Component of Basilar Membrane Vibration* for Various Combinations of Frequency and Level of the Primary Tones

Animal	Peak of BM response (kHz)	$2f_1-f_2$ (kHz)	f_1 (kHz)	f_2 (kHz)	SPL of f_1 (dB)	SPL of f_2 (dB)	Average level* of $2f_1-f_2$ relative to primaries (dB)
142L	21	18	20	22	112	111	-41
		20	22	24	111	110	-45
149L	20	16	18	20	101	101	-55
		16	18	20	111	111	-37
		20	22	24	100	99	< -60
		20	22	24	110	109	-54
153L	21	18	20	22	89	90	< -51
		18	20	22	99	100	-52
		20	22	24	90	94	-55
156R	23	20	22	24	103	102	< -50
		20	22	24	113	112	-41

* Expressed as the sound input which would have been required to give the observed level of vibration at $2f_1-f_2$.

Instrumental distortion including the sound generator was less than -60 dB so the figures represent distortion within the animal. At the lower levels measurement was generally limited by the noise level of the capacitive probe. The higher level of distortion at the higher SPLs is consistent with overloading distortion rather than an essential non-linearity.

In experiments at 90 and 100 dB SPL the level of $2f_1-f_2$ reached 55 dB below the primaries, about 40 dB below the level required to account for the psychophysical and neurophysiological results.

We have failed to find any significant component of basilar membrane vibration of frequency $2f_1-f_2$ in the first turn at 90 and 100 dB SPL. This result seems to conflict with the kind of model described earlier. There are other, less likely explanations for our results which do not conflict. (1) Our operative procedure may disturb the mechanism of the cochlea. This is unlikely as Evans¹⁰ has shown that the tuning curves of single cochlear nerve fibres in guinea-pig are unaffected by a similar procedure, and we verified that click threshold is substantially unaffected by opening and draining the first turn. Also, although measurements were never complete before the N_1 threshold had risen by 15 dB, there was no evidence that any of the mechanical parameters varied even for large changes in N_1 threshold. (2) There may be a species difference. The combination tone $2f_1-f_2$ has been demonstrated only in man (psychophysically) and cat (neurophysiologically). No other qualitative differences of structure or function have been found, however, and there is no reason to believe one exists for this phenomenon. (3) The combination tone $2f_1-f_2$ may not be present at the high frequencies necessary for measurements in the first turn. Goldstein¹, however, showed it to be little affected by frequency range, Goldstein and Kiang³ suggest that it is present throughout the audible range, and a response was observed to $2f_1-f_2$ at 27.5 kHz in a cell in cat cochlear nucleus (J. P. W. and E. F. Evans, unpublished data). (4) The sound intensities used may have been too high. Although overloading distortion increases with level there is some evidence that the proportion of $2f_1-f_2$ decreases slightly at higher levels, but this was not a great decrease. Goldstein¹ found the effect of increasing the primaries from 30 to 70 dB SL was only 2 to 6 dB, and Zwicker's¹¹ cancellation level for $f_1=1$ kHz, $f_2=1.3$ kHz at 80 and 90 dB SPL agrees with Goldstein's value at 50 dB SL and was about 10 dB less at 100 dB SPL. The synchrony of

nerve fibre response up to 100 dB SPL was found to be not strongly dependent upon stimulus level³.

Reconsidering models which involve pre-filtering, essential non-linearity and post-filtering, some modifications appear to be essential. First, the non-linearity cannot be hydrodynamic⁴ but may be in the coupling between basilar membrane and excitation of hair cells. Dallos¹², however, has shown that $2f_1 - f_2$ in cochlear microphonics does not vary appropriately with f_2/f_1 and sound level. Assuming that $2f_1 - f_2$ is present mechanically he concluded that the microphonic potential might not be an essential link in the peripheral transduction process. Our results make this conclusion unnecessary; cochlear microphonics do reflect the receptor potentials; the non-linearity would be central to the receptor potentials. Second, the second filter cannot be closely coupled to the basilar membrane but its location and nature have not yet been identified. Like the non-linearity, it presumably comes after the mechanical transduction process but must precede the generation of nerve impulses.

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Pituitary Inhibition of Pregnancy

HERE I report some new and unexpected findings on the role of the pituitary in the reproductive cycle of the tammar wallaby, *Macropus eugenii*. My results suggest a pituitary suppression of the corpus luteum during the seasonal embryonic diapause found in this species. They may have some relevance to the hormonal control of the analogous seasonal delayed implantation of eutherian species such as the badger, *Meles meles*¹; the roe deer, *Capreolus capreolus*²; and the black bear, *Ursus americanus*³.

The female tammar wallaby usually produces one young in January or early February. Mating occurs at a post partum oestrus, and the resulting blastocyst remains quiescent in the uterus and the corresponding corpus luteum is quiescent in the ovary until the next year⁴. The young suckles in the mother's pouch for up to 8 months and during this time the female remains in lactational anoestrus. Removal or loss of the pouch young during the breeding season causes resumption of development of the quiescent corpus luteum and embryo and a new young is born 27 days later. The wallaby does not breed between June and December, however, and removal of the pouch young during this seasonal anoestrus has no effect on the quiescent corpus luteum or embryo. They remain in "embryonic diapause"⁵ until the following breeding season,

when they resume development presumably in response to environmental stimuli⁶.

I have developed methods of hypophysectomy⁶ and radioimmunoassay for gonadotrophin⁷ to examine the influence of the pituitary on reproduction in wallabies which is suggested by these phenomena. The results obtained do not conform to the conventional concepts of pituitary activity in reproduction.

Groups of three wallabies were hypophysectomized at four-day intervals through pregnancy after removal of pouch young. Pregnancy continued, although hypophysectomy on the fourth day retarded embryonic growth, and the cycle of the corpus luteum of pregnancy also proceeded normally. Hypophysectomized animals, however, did not give birth; at autopsy they had dead full-term embryos in their uteri. Ovarian follicular growth and oestrous changes were suspended. In contrast, sham hypophysectomized control animals gave birth on the expected date and had a normal post partum oestrus and ovulation. Correspondingly, radioimmunoassay of peripheral plasma, taken daily after removal of pouch young from six intact wallabies through pregnancy and post partum oestrus, showed peaks of plasma gonadotrophin (11–18 ng ml.⁻¹) coincident with oestrus, but basal levels (2–5 ng ml.⁻¹) throughout pregnancy. The pituitary therefore was not essential for any stage of the pregnancy that followed removal of pouch young.

A group of wallabies was hypophysectomized before removal of pouch young (during lactational anoestrus). Lactation ceased within two days, although young remained on the teats for a further two to five days. The quiescent corpora lutea and blastocysts of these animals resumed development and pregnancy went to term. This suggested that resumption of embryonic development was independent of pituitary stimulus, but might be governed by lactation inhibition during the breeding season. To test this, six lactating wallabies were hypophysectomized in the non-breeding season. Lactation again ceased within two days, and the quiescent corpora lutea and blastocysts resumed development. Sham hypophysectomized controls showed no development. It seemed that the corpus luteum or blastocyst was being suppressed either by the mammary gland or by the pituitary. Seven non-lactating wallabies were therefore hypophysectomized in the non-breeding season. Corpora lutea and embryos again resumed development. Inhibition was evidently due either directly or indirectly to the pituitary.

The influence of the mammalian pituitary gland in pregnancy has been studied in about twelve eutherian species⁸. In all except the guinea-pig, *Cavia porcellus*⁹, hypophysectomy before implantation caused death of the embryo. Most species were intolerant of the operation before the second half of pregnancy, while in the dog, *Canis canis*, and rabbit, *Oryctolagus cuniculus*, embryos died when hypophysectomy was performed at any stage⁸. The guinea-pig is unique among eutherian species studied in that the corpus luteum of pregnancy continued to grow after hypophysectomy⁹; in others it degenerated. The effect of complete hypophysectomy on the parturition of eutherians is not clear, but several species are reported to have produced live young when hypophysectomized late in pregnancy⁸. Termination of lactation, follicular growth and normal cyclical oestrus after hypophysectomy are general among the eutherian species that have been studied, and the hypophysectomized wallaby is no exception in this.

The unexpected resumption of embryonic development in non-lactating wallabies hypophysectomized in the non-breeding season suggests a pituitary inhibition of the corpus luteum. If so, embryonic diapause in the normal wallaby is presumably ended by removal of a pituitary inhibition, and not by gonadotrophic action. A similar mechanism may be involved in delayed implantation of eutherian species. A pituitary inhibition of embryonic development during delayed implantation in badgers would explain why even massive injections of gonadotrophins fail to terminate delayed implantation in that species¹. The inhibitory role may be played by oxytocin,

as workers on eutherian species have demonstrated partial or total inhibition of the corpus luteum by oxytocin¹⁰⁻¹¹, and oxytocin inhibits development of the corpus luteum of red kangaroos after removal of pouch young¹².

Photoperiod is probably an important controlling factor. The delayed blastocyst of the badger will implant under experimentally reversed photic and climatic conditions¹³ and the tamar wallaby reverses its breeding season when translocated to the northern hemisphere¹⁴.

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The Expression of Female Sex-limited Characters in the Male

DESPITE very different reproductive characteristics of males and females, autosomal genes controlling these characters are common to both sexes. (The transmission of genes for female fertility through the males is demonstrated by differences in the reproductive performance of the daughters of different sires^{1,2}.) Furthermore, the fact that the same gonadotrophic hormones are intermediaries in the physiological control of reproductive activity in both sexes is well established³. The quantitative expression of sexual activity in males and females may therefore be genetically correlated, a possibility which is supported by the observation that breed and seasonal differences in ovine female fertility are associated with differences in male libido⁴.

In view of the limitations to the selection differential which can be applied to sex limited characters such as fertility, potential relationships between male and female reproductive activity were investigated in two species, the mouse and the sheep. In the mouse study, the testis weight of male mice was examined in lines in which the ovulation rate had been changed following twelve generations of direct selection for natural or induced ovulation rate¹; and in the sheep work, the growth of the testis was examined in two breeds, the Finnish Landrace and the Tasmanian Merino, which have more than a three-fold difference in litter size⁵, and in the cross between them.

The males of the last selected generation of the four mouse strains, and the control, were killed at approximately 10 weeks of age, and their testes removed and weighed. The mean testis weight of the control and four selected lines was then expressed as the mean of the 16 family means which made up that line, in the same way that the mean of family means was used to measure body weight and ovulation rate. These mean testis weights are given in Table 1 together with the body weights of these individuals at the time of slaughter and the natural ovulation rate of their primiparous female contemporaries. Body weight and testis weight are seen to have changed in the same direction as the ovulation rate, the divergence of testis weight between the lines selected for both high and low natural and high and low induced ovulation rate being statistically significant ($P < 0.05$). The relationships between body weight and ovulation rate have been described earlier⁶.

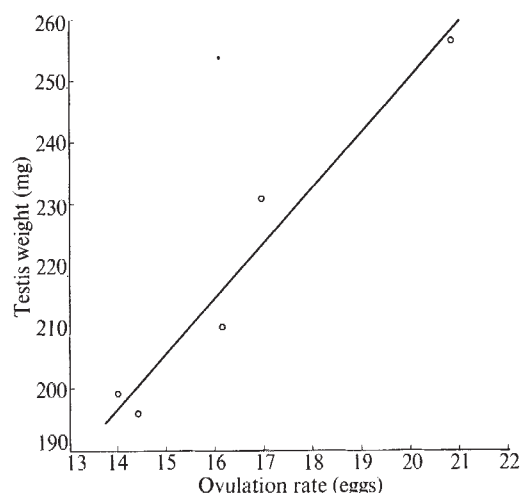


Fig. 1 Relationship between mean testis weight and mean ovulation rate of five mouse lines selected for ovulation rate. The between line regression of testis weight on ovulation rate was: testis wt. (mg) = $70 + 9 \times$ ovulation rate.

The change in testis weight following selection for ovulation rate indicates that the expression of the two traits may be genetically correlated and this relationship is illustrated in Fig. 1 as the mean testis weight of the lines plotted against their mean natural ovulation rate. The correlation between them is 0.97 and the genetic regression of testis weight on ovulation rate as estimated from the observed divergence of the high and low natural lines, 8.4 mg per egg. Although part of the change in testis weight may have arisen from changes in body weight, the partial correlation between ovulation rate and testis weight for constant body weight was 0.82, indicating that the removal of variation in body weight had only a small effect on the magnitude of the correlation between ovulation rate and testis weight. This indicates that ovulation rate and testis weight are genetically correlated in the mouse population.

Table 1 Mean Ovulation Rates and Testis Weights of Selected Mouse Lines

Line	Body weight (g) (C. 10 wks)	Ovulation rate	Testis weight (mg)
High natural	34.72 $\pm$ 3.66	20.87 $\pm$ 0.54	257 $\pm$ 22.0
Low natural	29.68 $\pm$ 1.18	13.98 $\pm$ 0.54	199 $\pm$ 22.1
High induced	31.48 $\pm$ 2.61	16.84 $\pm$ 0.73	231 $\pm$ 23.2
Low induced	27.37 $\pm$ 2.12	14.41 $\pm$ 0.50	196 $\pm$ 21.6
Control	31.27 $\pm$ 1.66	16.13 $\pm$ 0.45	210 $\pm$ 19.1

studied and that this correlation is not solely mediated through body weight; the magnitude of the genetic correlation is, however, dependent upon several factors, including the heritability of testis weight for which no estimates are available.

The growth rate of the testes of the sheep was examined in terms of the testis diameter of pure and crossbred males, measured at weekly intervals from 8 to 26 weeks of age over the period of rapid growth⁷. The testis diameter of ten Finn, four Merino and nine crossbred males was measured as the difference between the thickness of a fold of scrotal skin and the overall diameter of the testis and two layers of skin. The $\log_e$ mean weekly testis diameter is plotted against the $\log_e$ mean weekly body weight in Fig. 2; the regression coefficients are 2.16 mm kg⁻¹ for the Finnish Landrace males, 2.65 for the crossbreds, and 2.27 for the Tasmanian Merino males, the crossbred regression coefficient being significantly greater ($P < 0.05$) than that of the pure breeds. The growth rate of the testes was therefore similar in the two pure breeds, but their mean diameters were significantly different—that of the Finnish Landraces exceeding that of the Merinos throughout the study. As the testes of the crossbred males grew faster in diameter than those of the parental breeds, they showed heterosis in this character such that at $\log_e$ body weight = 3.2 they exceeded those of the larger parent.

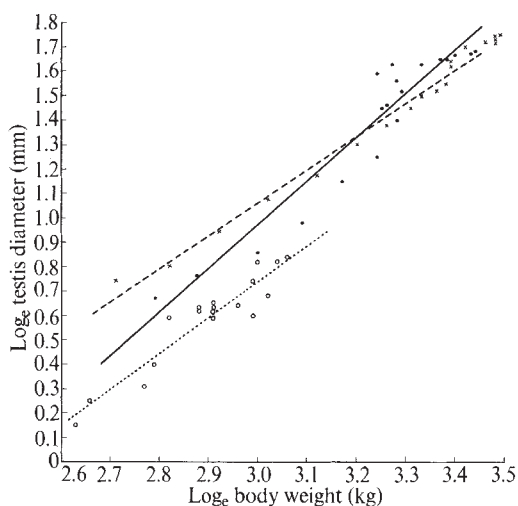


Fig. 2 Increase in testis diameter relative to body weight in Finnish Landrace (x - - - x), Tasmanian Merino (O . . . O) and Merino (♂) cross Finn males (● - - - ●) from 8 to 26 weeks of age.

In both sheep and mice, testis weight or diameter was greater in the line or breed with the higher ovulation rate, indicating that the quantitative expression of reproductive characteristics in male and female mammals is genetically correlated. This correlation may, of course, be mediated *via* one or more of several physiological systems, and although the present experiments do not help to differentiate among these, between breed studies (refs. 8 and 9 and Land, Pelletier, Thimonier, and Mauléon, in preparation) of pituitary and plasma gonadotrophin activity have indicated that differences in sheep fertility may be related to differences in LH activity. Regardless of the physiological pathway underlying the genetic correlation, this correlation indicates that it might be possible to base genetic selection for increased fertility—or some measures of fertility—in the female on the reproductive characteristics of the male. More generally, measures of fertility in either sex may not be wholly sex limited.

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IQ Evoked Responses and Fourier Analysis

CONFLICTING results have been obtained in studies¹⁻³ dealing with Fourier analysis of evoked responses (ER) in relation to human intelligence. On rethinking, I realized (a) that the spectral characteristics of ER change with time, and (b) that most of the differences between high and low IQ subjects appear to occur in the first 150 ms of the response.

Previously Fourier analyses looked at a 0.5 s slice of ER which does not take into account its non-stationary properties. I have, therefore, applied a technique used in speech recognition^{4,5} in which Fourier analysis is performed in a sliding time window. Zero padding is used to improve definition of the spectral peaks^{6,7}.

From an FM tape library of over 1,000 subjects, I selected a high and a low IQ group on the following basis: IQ scores of 120 plus on two out of three IQ tests given—the Otis, PMA and WISC yielding 92 high IQ subjects. The low IQ group had IQs of 85 or less on two out of the three tests yielding 71 subjects. The total sample of 164 represents all subjects available to me who met these criteria. The mean age of the subjects was 11.6 years, with a range of 7.5 to 15.3 years. Of the total, ninety-two were males and seventy-two were females. All of these subjects were from the school population and all have had routine medical examinations; none of the subjects had diagnosed organic illness of any sort.

Data on FM tapes were as follows. The EEG was derived from bipolar scalp contact electrodes 6 cm apart astride C₄ in the 10-20 system. The EEG was recorded in a bandwidth 3 dB down at 3 and 50 Hz with an attenuation slope of 24 dB per octave. Four hundred trigger pulses corresponding to the onset of photic stimuli were also recorded. The data were converted to digital form (250 Hz sampling rate, 6 bit amplitude quantization) and a conventional amplitude average was computed over 400 stimuli using the first 60 sample points representing 240 ms of record. Fourier analysis was then performed in an 80 ms time window which was incremented in 8 ms steps from 40-200 ms. Frequency definition was 1 Hz (using zero padding) from 12-50 Hz. The average amplitude of the Fourier transform of the high and low IQ subjects was computed for each time increment and at each frequency in steps of 1 Hz from 12-50 Hz resulting in a two-dimensional matrix of Fourier amplitude coefficients with time and frequency as the independent variables.

The coefficient matrices for subjects in the high and low IQ groups have been averaged over each group. For each cell, *t*-tests were computed for the frequency *versus* time shift matrix of high and low IQ subjects. A coherent and statistically significant band of *t* values was obtained indicating that the group of high IQ subjects have significantly more energy content in the frequency band of 18-29 Hz at 88 to 136 ms following the stimulus (Table 1). It is interesting to note that

Table 1 Frequency and Time Shift where High IQ Subjects Have Significantly ($P \geq 0.05$) Greater Energy Content in Their Fourier Transform Than Low IQ Subjects

Frequency (Hz)	Window centre position (ms)
12	136, 176
13	136, 176
14	136, 176
15	136, 176
16	136, 176
17	136, 176
18	104, 128, 136
19	104, 128, 136
20	104, 112, 120, 128, 136
21	104, 112, 120, 128, 136
22	96, 104, 112, 120, 128
23	96, 104, 112, 120, 128
24	88, 96, 104, 112, 120, 128, 152
25	88, 96, 104, 112, 120, 128, 152
26	88, 96, 104, 112, 120, 128
27	88, 96, 112, 120
28	88, 96, 112, 120
29	88, 96, 112
30	88
31	88
32	88
33	— — —
34	— — —
35	— — —
36	— — —
37	160
38	152, 160
39	152, 160
40	152, 160
41	120
42	120
43	64
44	64, 176
45	64, 176
46	64, 96
47	64, 96
48	64, 96
49	56, 64
50	56, 64

Fourier analysis was done with an 80 ms time window incremented in 8 ms steps from 40–200 ms.

in all cells where significant t values were obtained, high IQ subjects always had more energy content than low IQ subjects.

Although many significant t values were obtained, it is difficult to classify individuals based on these mean differences. Therefore, this technique by itself is not useful clinically. It is, however, important to know the nature of the individual differences in the spectrum of ER. The use of Fourier analysis over long time windows is inappropriate when applied to non-stationary phenomena, such as the ER, and this may explain the conflicting results obtained by the previous authors cited.

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New Aspects of *Salmonella* Infection in Broiler Production

POULTRY are a considerable reservoir of salmonellae and a common source of infection in human *Salmonella* epidemics¹. In 1971 a severe outbreak of *Salmonella infantis* infection occurred among Finnish broiler flocks, and 277 human cases² were diagnosed as caused by the same serotype. The broiler industry suffered serious losses through confiscation of broiler meat and elimination of infected birds.

As broiler production is carried out under abnormally hygienic conditions we suspected that the prevalence of *Salmonella* infection in broilers as compared to layer hens was caused by a hampered development of the intestinal flora, which normally participates in defence against pathogenic bacteria.

We present here the preliminary results of experiments studying the way in which normal adult gut contents given to baby chicks affect their susceptibility to *S. infantis* infection. These chicks were compared to similar chicks in conditions simulating the normal broiler house. The test animals were offspring of Cornish cocks and Pilch de Kalb hens hatched in a commercial broiler hatchery. The egg shells were disinfected with formalin gas. At the age of 1–2 days, 26 chicks received 0.5 ml. of gut contents (ingesta) from adult cocks, diluted with physiological saline 1:10 (w/v). Ingesta were taken from the crop and intestinal tract of three healthy cocks and were placed directly in the chick crops. Another group of twenty-four chicks served as controls. The groups were reared separately and all the birds were held in isolation. One day after the first treatment, both groups were divided into two subgroups. One of the subgroups was given approximately 10^3 and the other 10^6 *S. infantis* isolated from a field case (Table 1) into the crop.

Feeding began immediately after introduction of *Salmonella*. The feed did not contain antibiotics, chemotherapeutics or coccidiostats. Chicks were killed when they were 8–22 days old, using ether inhalation. The same number of birds from each group were killed at the same time. A bacteriological examination was performed on the crop, the small intestine and the caeca (Tables 1 and 2).

All the birds in control groups C and D (Tables 1 and 2) remained *S. infantis* carriers. In groups A and B which received ingesta before infection, 77% of the group given the smaller inoculum and 69% given the larger were free from *S. infantis*.

Table 1 shows the numbers of *Salmonella*-positive samples and Table 2 the numbers of salmonellae. During the experiment one bird died in each of groups C and D, and were excluded from the tables. Both chicks had more than 10^6 salmonellae per g in the caecal contents.

These results clearly demonstrated the inhibitory effect of ingesta on the establishing of *S. infantis* in the broiler gut.

The large numbers of salmonellae (10^6 – 10^9 g⁻¹) found in the caeca of birds without pretreatment point to a possible preferential colonization in that part of the intestine. *S. infantis* has most often been found in caeca⁵. In those pretreated birds which had salmonellae, the numbers were very low. In birds without pretreatment the numbers of salmonellae recovered from the crop and small intestine showed great variation. The experiment was subsequently successfully repeated.

Table 1 Occurrence of *Salmonella infantis* in Contents of Crops, Small Intestines, and Caeca of Experimentally Infected Chickens

Group	Pretreatment of chickens 1-2 days old	Number of <i>S. infantis</i> given orally*	Number of chickens examined	Birds with salmonellae recovered from different sections of the digestive tract							
				Crop		Small intestine		Caeca		At least one sample positive	
				No.	%	No.	%	No.	%	No.	%
A	Pretreatment	10 ³	13	2	16	1	8	0	0	3	23
B	Pretreatment	10 ⁶	13	3	23	1	8	1	8	4	31
C	No pretreatment	10 ³	12	7	58	7	58	12	100	12	100
D	No pretreatment	10 ⁶	10	8	80	6	60	10	100	10	100

* Numbers of salmonellae given are approximate.

Grouping made according to pretreatment with contents of the digestive tract and doses of salmonellae.

Bromthymol blue-lactose-saccharose-agar, triple-sugar-agar and urea-agar were used as culture media. Selenite broth was used for enrichment. All the substrates were produced by Orion Pharmaceutical Co., Helsinki, Finland. The sera employed for typing were produced by Behringwerke AG, Marburglahn, Germany.

On the basis of these results we wish to put forward the hypothesis that as the development of the intestinal flora is delayed when broilers are produced in a *milieu* low in microbes, the newly hatched birds are in a transitional state between germ-free and normal animals for a few days. The defence against bacteria entering the body through the mucosa or colonizing on it is probably weaker than normal. This state is remedied by administering intestinal contents or even bacterial cultures from older chicks. The chicks' resistance against salmonellae increases with age⁴, so it is important to protect the very young birds.

If *Salmonella* contamination occurs in the dry feed, the number of bacteria is usually very low. However, small numbers of salmonellae may be able to colonize the intestine of a very young chick but not of an older bird. A bird with salmonellae multiplying in the intestine is a dangerous carrier,

effectively spreading the disease. This creates serious environmental and food-hygiene problems.

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Table 2 Effect of Pretreatment and Inoculum Dose on Occurrence of *Salmonella infantis* in Three Sections of the Digestive Tract

Source of the sample	Number of salmonellae, g ⁻¹	No. of chickens grouped by pretreatment and salmonellae dose			
		Pretreated		No pretreatment	
		10 ³ * (group A)	10 ⁶ (B)	10 ³ (C)	10 ⁶ (D)
Crop	10 ⁶	0	0	1	1
	10 ⁵	0	0	1	1
	10 ⁴	0	0	0	1
	10 ³	0	0	2	2
	Only by enrichment	2	3	3	3
	Not detected	11	10	5	2
Total		13	13	12	10
Small intestine	10 ⁶	0	0	0	1
	10 ⁵	0	0	1	0
	10 ⁴	0	0	0	1
	10 ³	0	0	0	3
	10 ²	0	0	0	1
	Only by enrichment	1	1	6	0
Total	Not detected	12	12	5	4
		13	13	12	10
Caeca	10 ⁸	0	0	4	4
	10 ⁷	0	0	3	3
	10 ⁶	0	0	5	3
	Only by enrichment	0	1	0	0
	Not detected	13	12	0	0
Total		13	13	12	10

* Numbers of salmonellae given are approximate.

Semi-dwarf Wheats induced by Monosomy and Associated Changes in Gibberellin Levels

THE genetic basis of height control in the hexaploid wheats, *Triticum aestivum* (2n=6x=42), is complex as shown by the study of inter-varietal chromosome substitution lines¹ and monosomic analyses². Almost all twenty-one pairs of chromosomes have some effect on plant height in the tall and medium-tall varieties.

The genetic sources used in breeding dwarf and semi-dwarf varieties now popular in agriculture are "major gene" dwarfs, the most widely used being the Japanese variety, Norin 10. This dwarfism was thought to be controlled by two duplicate loci but the F₂ segregation obtained from a cross between the varieties Norin 10-Brevor 14 and Chinese Spring showed a wide range of phenotypes³. Monosomic analyses of the same varietal difference have implicated at least eleven chromosomes, the loss of chromosomes 2A, 2B, 2D, 3D, 4B and 4D especially resulting in a lowering of plant height.

The physiological cause of dwarfing in wheat is unknown although gibberellin (GA) metabolism is almost certainly involved. Unlike tall wheat varieties only small changes in final plant height of Norin 10-Brevor 14 occur when GA is applied⁴. Radley⁵ has shown that a number of semi-dwarfs derived from Norin 10 have very high levels of free endogenous

GA compared with tall controls. Both these results suggest a block in utilization of GA in these lines.

A form of semi-dwarfism similar in morphology to that of Norin 10 is found among aneuploid lines of the semi-winter wheat variety Bersee. In a series of monosomics in which each chromosome pair in turn is represented only by a single chromosome, there are differences in height which may be ascribed to the dosage effects of individual chromosomes. The monosomics for chromosomes 2A, 2B and 2D, representing corresponding homoeologous chromosomes derived from each of the three different diploid ancestors of wheat, and carrying out similar genetic activities, are outstanding. Under glass-house conditions they are 30% shorter than euploid in final plant height. Other features of the Norin 10 dwarfs are also apparent. Compared with the euploid phenotype, coleoptile lengths are reduced and leaves are wider and darker. Unlike the Norin 10 dwarfs, however, these monosomics respond to applied GA and to the inhibitor ethyl trimethyl ammonium chloride (CCC), which blocks the endogenous synthesis of GA (Table 1). Treatment with GA produces a larger percentage increase in the height of seedlings in the group 2 monosomics than in Bersee euploid or in the controls monosomic for other chromosomes.

Table 1 Height of Seedlings at 15 Days and Responses to GA₃ (10 µg ml.⁻¹) and CCC (100 µg ml.⁻¹)

	Control	+GA ₃ Response and % control value	+CCC
Euploid	24.87 cms	+9.3 (137.4%)	-9.1
Bersee 2A	20.94*	+9.6 (145.6%)*	-8.2
Bersee 2B	22.96	+9.6 (141.7%)	-8.5
Bersee 2D	21.80*	+10.1 (146.2%)*	-8.3
Bersee 5BS 7BS	23.85	+9.3 (139.1%)	-9.6
L.S.D. 5%	2.71 cms	(5.5%)	

* Significant difference from the euploid value.

The growth regulators were supplied in Hoagland's solution and the seedlings grown in continuous light at 18°C.

In two further experiments the free endogenous GA was extracted and measured in seedlings of Bersee group 2 monosomics. Table 2 shows that in both experiments the Bersee group 2 monosomics have lower levels of total free GA and that this variation is not an inevitable consequence of monosomy.

Table 2 Free Endogenous Gibberellin-like Substances Extracted from 14 Day Old Seedlings

	I	II	Final height 1971
Bersee euploid	2,310	1,570	130 cm
Bersee 2A	885*	625*	98 cm
Bersee 2B	1,210*	675*	99 cm
Bersee 2D	755*	—	98 cm
A and B genome monosomics excluding group 2	—	1,273	

* Significant difference from the euploid value.

Plant material was homogenized in buffer at pH 8.6 and extracted with ethyl acetate at pH 3.0. This acid fraction was concentrated and chromatographed on paper in isopropanol, NH₃, water (10 : 1 : 1) and the RF segments containing GA₁ + GA₃-like activity were tested in the barley endosperm bioassay⁶. Two replicate extractions were assayed in experiment I and three replicates of all A and B genome monosomics in experiment II. Total GA is expressed as GA₃ pg equivalents gm⁻¹ fr. wt.

These results could be explained if the group 2 chromosomes control some part of the metabolism of GA, probably biosynthesis and, in the monosomic condition, limit the availability of GA. This would produce similar effects in plant height to

the block in utilization postulated for the Norin 10 based semi-dwarfs.

The differences in GA levels between the two kinds of semi-dwarf and also their different responses to applied GA and CCC could indicate that the underlying genetic causes of the Norin 10 dwarfing are not associated with allelic variation among loci carried on the group 2 chromosomes. Although monosomic analyses involving semi-dwarf wheats as one of the parents³ have implicated these chromosomes in the control of height differences, it must be remembered that such analyses confound both allelic and chromosome dosage differences. Under such circumstances the group 2 chromosomes are likely to feature markedly although allelic variation may not be present.

Another observation possibly related to the influence of group 2 chromosomes on GA levels concerns investigations into plant survival in response to freezing among monosomic lines of the variety Cappelle-Desprez⁷. It has been shown^{8,9} that application of GA by soaking the seeds or by spraying the seedlings of tall varieties stimulates growth and reduces the level of cold resistance. Similarly, treatment with CCC, presumably by lowering endogenous levels of GA, increases the level of cold resistance. Among the monosomics of Cappelle-Desprez tested for their cold resistance the only representative of group 2 tested, chromosome 2A, showed increased resistance; it is probable that this increased resistance could be due to the low levels of GA brought about by the reduction in chromosome dosage. Further support for this effect of chromosome dosage derives from the analysis of cold resistance among inter-varietal chromosome substitution lines of Cappelle-Desprez in a Chinese Spring background¹⁰ in which the effect of chromosome 2A in the disomic state was not apparent.

It seems likely that in wheat, semi-dwarfism may be accomplished either by reducing synthesis of GA or by reducing the availability of the GA present as in the Norin 10 based dwarfs. The different levels of GA present as free endogenous hormone which are a consequence of these two types of height control may affect developmental processes other than height. It is possible from an understanding of these differences that an explanation of the recent agricultural successes of the semi-dwarf wheats will be found.

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Tool-use in Free-living Baboons in the Gombe National Park, Tanzania

AN animal may be said to use a tool when an object is used as a functional extension of the hand (or claw, mouth, beak) in the attainment of an immediate goal¹. References have been made to tool-using in wild baboons. Marais² describes the manner in which the individuals of one troop of baboons



Fig. 1 Clover (3 yr old female baboon) using a stone to wipe her mouth after feeding. (Photo: C. Packer)

(*Papio ursinus*) broke open the hard-shelled fruits of "sausage trees" (*Kigelia pinnata*) with rocks in order to feed on the seeds. Kortlandt and Kooij³ mention one baboon using a stone to squash a scorpion to eat it, and another using a stick to prod about in a termite nest. In these two instances no details of the behaviours nor of the conditions under which they were observed are given so that they should probably not be accepted as wholly reliable.

There are few reports of tool-use in captive baboons. Bolwig⁴ describes one individual (*P. ursinus*) who used a stick to rake in food out of reach; he recorded purposefully aimed throwing in the same baboon. Kortlandt and Kooij³ collected twelve reports of apparently aimed throwing in zoo baboons.

Recently a new tool-using performance has been twice observed in the olive baboons (*P. anubis*) at the Gombe National Park, Tanzania, involving the use of an object for wiping the mouth. To feed on the seeds of *Diplorhynchus condylocarpon* the baboons must break open the tough-shelled pod in which the seeds are surrounded by a white glutinous juice drying to form a rubbery cement. When this food is in season, the baboons are frequently seen with the hair around their lips matted by the juice to which pieces of pod or leaf often adhere. Often they will pause to rub their dirty mouths against a tree trunk or large rock.

One three-year-old female, however, picked up a fairly large stone with which she repeatedly and forcefully rubbed at her muzzle in an attempt to remove the dried juice (Fig. 1).

The second incident, observed about one month later, involved an adult male of a different (neighbouring) troop. He cut his lower lip during a fight, causing blood and saliva to run down his chin. When, presumably, the blood had begun to coagulate, the baboon looked around, reached out for an old piece of maize kernel (the troop was foraging around human habitation) and with this gently wiped away most of the blood from his lower lip. Whilst we cannot draw conclusions from one episode it is interesting that, although there were several stones nearby, this baboon selected a softer, more porous and generally more suitable material.

The only other non-human primate which has been observed using objects as "napkins" in this way is the chimpanzee^{1,3}. At the Gombe Stream the chimpanzees typically use leaves to wipe dirt or blood from their bodies. Like the baboons, they also feed on the seeds of *Diplorhynchus condylocarpon* but, whilst they have been observed to rub their mouths against tree trunks and to indulge in a variety of facial contortions in an endeavour to remove the juice around their mouths with their lips and teeth, they have not, to date, been observed to use any objects as wiping tools in this context.

Admittedly we have only observed two individual baboons using objects in this way but because each belongs to a different troop, and because after the first event relatively little time elapsed before the behaviour was noticed again, it may occur fairly commonly. It is an action which would be extremely easy to overlook, and we draw attention to it here in order that other scientists working in the field may be aware of this behaviour. The research was financed by the Grant Foundation and Stanford University.

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Effect of Seawater Soluble Fraction of Kerosene on Chemotaxis in a Marine Snail, *Nassarius obsoletus*

IN aquatic environments, behaviour such as food finding, reproductive activities and orientation may depend on an organism's detection of specific chemical cues. One of the dangers of the increasing load of pollutants in the sea is that natural chemically mediated behaviour important in the lives of marine animals may be disrupted by minute amounts of these contaminants. This possibility has been suggested for chronic pollution by oil, whose sublethal effects are little known¹. To examine quantitatively behavioural disruption by oils, we have investigated the effect of the seawater-soluble fraction of kerosene on the attraction of the marine prosobranch snail, *Nassarius obsoletus*, to food extracts. This snail has already been found to be quite suitable for studies of chemoreception^{2,3}. Kerosene seawater extract was selected because its components have been identified and quantified and are also present in seawater extracts of some crude oils⁴.

Nassarius obsoletus were collected in the autumn of 1970 and spring of 1971 at several locations on Cape Cod, Massachusetts. During the autumn, they were held in flowing seawater at 14–15° C and fed occasionally on crushed *Mytilus edulis* for 6 weeks before experiment 1. During the spring, *N. obsoletus* were kept in heated seawater at 14–15° C without food for 10 days before experiment 2.

The experiments were performed in an acrylic plastic Y-chamber with equal angles between the arms, which were 42 cm long and 10 cm high and wide. Two upstream arms received seawater flowing at 1.5 l. min⁻¹ (14–15° C); one was selected randomly as the stimulus arm for each trial. In this arm, seawater was pre-mixed with stimulus solutions in the first of two mixing chambers. A rhodamine B flow profile

indicated that complete mixing occurred in the stimulus arm. In the downstream arm, mixing was incomplete although laminar flow was rarely observed.

Seawater extract of kerosene was prepared as described by Boylan and Tripp⁴ by slowly stirring a layer of 25 ml. kerosene (Gulf Oil Co. grade) over 1.5 l. seawater for 12 h, and separating the aqueous and oil layers. The seawater-soluble kerosene extract, consisting mostly of benzenes and naphthalenes, was calculated to contain about 610 µg of oil/l. seawater⁴. The kerosene extract used in experiment 1 was prepared in this manner. For experiment 2, kerosene seawater solubles were extracted with pentane four times, the pentane removed by rotary evaporation, and the residue mixed with 300 ml. seawater, in an attempt to concentrate the extract.

In experiment 1, there were four treatments: attractant, seawater control, kerosene extract and attractant plus kerosene extract. The attractant, oyster (*Crassostrea virginica*) extract, was prepared by Soxhlet extraction of the soft parts and extra pallial fluids with methanol for 6 h, filtering (0.45 µm 'Millipore'), and concentrating *in vacuo*. The concentrated extract was desalted on a 'Dowex 50 W-X12/1-X8' column equilibrated with ammonium bicarbonate. The nonretained effluent was freed from ammonium bicarbonate by repeated evaporation *in vacuo* and stored at -20° C. The quantity of extract obtained was 1 ml./1.6 oysters. Stimulus solutions for attractant and attractant plus kerosene treatments were prepared by diluting 0.0125 ml. of oyster extract in 110 ml. seawater or kerosene seawater extract. Equal volumes of seawater or kerosene extract alone were used in control and kerosene treatments. These solutions flowed at a constant rate of 2.5 ml. min⁻¹ from a flask inverted into a syringe fitted with a No. 20 needle. The concentration in the stimulus arm of kerosene extract was calculated at 1 part 10⁻⁹ (p.p.b.), and of oyster extract (wet weight basis) about 0.33 parts 10⁻⁶ (p.p.m.).

Table 1 Experiment 1

	Attractant (oyster extract)	Attractant+ kerosene extract (1 p.p.b.)	Kerosene extract (1 p.p.b.)	Control (seawater)
Stimulus arm $\bar{x} \pm \text{s.e.}$	4.4 ± 1.0	1.7 ± 0.3	1.0 ± 0.5	0.8 ± 0.8
Nonstimulus arm $\bar{x} \pm \text{s.e.}$	0.6 ± 0.4	1.3 ± 0.3	1.7 ± 0.7	0.3 ± 0.3
Number of trials (ten snails per trial)	5	3	3	4

Mean $\pm$ s.e. of number of *N. obsoletus* in stimulus and nonstimulus arms in each treatment at 15 min. Means joined by underlining are not significantly different at or below the 0.05 protection level using Kramer's modification of Duncan's multiple range test⁵.

Experiment 2 included the same four treatments. An attractant was prepared from homogenized adductor muscle of the scallop *Aequipecten irradians* (2 g in 100 ml. seawater, filtered through a 'Pyrex' glass filter and refrigerated). All tests were run within 1.75 h of preparation of the attractant. For the attractant and attractant plus kerosene treatments, 5 ml. of scallop homogenate was diluted with 45 ml. of seawater or kerosene extract respectively; 5 ml. of seawater was added in the control and kerosene experiments. These solutions were introduced at 2.3 ml. min⁻¹. Concentrations of kerosene extract and scallop homogenate (wet weight basis) were calculated at 4 p.p.b. and 3 p.p.m. respectively.

The attractant and control treatments were run before the experiments in which kerosene extract was used, due to possible adsorption of small amounts of kerosene components on the walls of the Y-chamber and glass. In experiment 2, the chamber was scrubbed with hot water following each trial to remove slime trails and adsorbed kerosene components.

The experiments differed slightly in procedure. In the first experiment, ten specimens of *N. obsoletus* were lowered gently in a Petri dish into the base of the lower arm 2 min after introduction of the stimulus solution. The position of the snails was recorded at 5 min intervals for 30 min. In the second experiment, each trial lasted 15 min; the snails were introduced halfway up the lower arm. Those reaching a line halfway up either upper arm were removed with forceps, scored as responding positively, and the time noted. In all experiments, snails were used for one trial only.

Table 1 shows the results of experiment 1, 15 min after the introduction of snails, when the mean number attracted to oyster extract alone reached an asymptote. In the presence of 1 p.p.b. kerosene extract, attraction to oyster extract was reduced to a level not significantly different from the control or kerosene treatments. The number of snails observed in the nonstimulus arm did not differ significantly between treatments. In all treatments, half or more of the snails remained in the lower arm; most crawled about actively. No difference in behaviour was observed between snails exposed to kerosene extract alone or to seawater. Although *N. obsoletus* can follow mucus trails of conspecifics², at least 50% of the snails in the attractant treatments did not follow the other snails into the upper arms; in the other treatments, snails entered either arm about equally (Table 1).

Table 2 Experiment 2

	Attractant (scallop homogenate)	Attractant+ kerosene extract (4 p.p.b.)	Kerosene extract (4 p.p.b.)	Control (seawater)
Stimulus arm $\bar{x} \pm \text{s.e.}$	7.0 ± 0	4.8 ± 0.9	0 ± 0	0.6 ± 0.6
Nonstimulus arm $\bar{x} \pm \text{s.e.}$	0.2 ± 0.2	0 ± 0	0 ± 0	0.3 ± 0.3
Number of trials (ten snails per trial)	5	4	2	3

Mean $\pm$ standard error of number of *N. obsoletus* responding positively in stimulus and nonstimulus arms in each treatment. Comparison of the attractant and attractant plus kerosene treatment means using the Mann-Whitney U test⁸ showed that they were significantly different below the 0.05 level.

As Table 2 shows, in experiment 2 there was a significant reduction in attraction to scallop homogenate in the presence of kerosene extract. Results of a previous experiment of the same format had shown that kerosene extract at 0.8 p.p.b. was not similarly effective.

Seawater extracts of several crude oils have low boiling components similar to those in kerosene seawater extract⁴. Although little has been published on concentrations of oil in seawater, values for dissolved Bunker C oil have been determined 3 months after a spill in Chedabucto Bay, Nova Scotia. Concentrations for stations up to 20 miles from the spill site ranged from less than 10 to 90 p.p.b.⁷. Naphthalenes, the major components present in kerosene seawater extract, have been reported in concentrations around 1 p.p.b. in the polluted Charles River Basin, Boston. Naphthalenes are known to be acutely toxic^{4,8} and may have altered the behaviour of *N. obsoletus* in the present experiments, perhaps through interference with chemoreception.

The two experiments reported here demonstrate that minute quantities of the seawater-soluble fraction of kerosene interfere with the chemically mediated attraction to food extracts in *Nassarius obsoletus*. Oil components may disrupt chemically mediated behaviour in organisms of widely separated taxa: dilute concentrations of diethyl ether extracts of crude oils inhibit the feeding response and sex pheromone response in the lined shore crab, *Pachygrapsus crassipes*⁹. Our results suggest that there is need for further research on the effects of oil pollutants on chemically mediated behaviour of marine animals.

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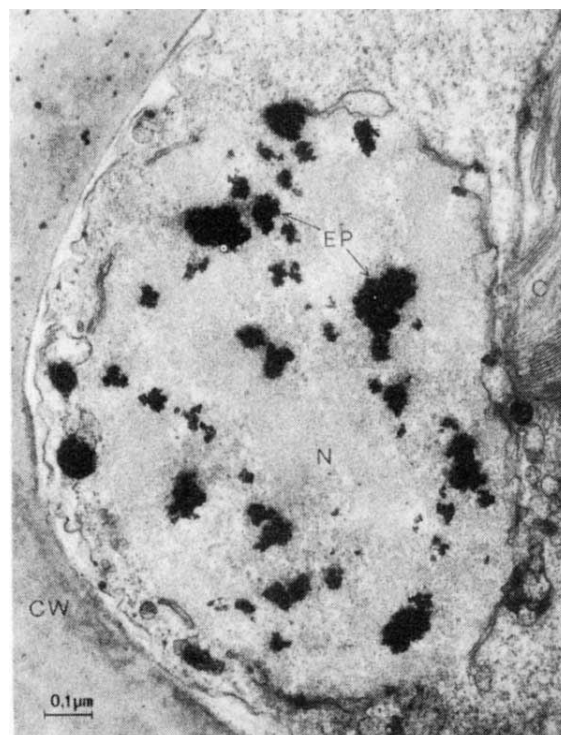


Fig. 1 Electron micrograph of leaf cell of *Rhytidiadelphus squarrosus*, collected from a park near a busy main road. The nucleus contains many electron-dense particles shown by means of 'EMMA' to contain lead. The nuclear membrane is damaged. The section has not been post-stained. CW, Cell wall; C, chloroplast; EP, electron-dense particles shown by X-ray microanalysis to contain lead; N, nucleus.

Lead Accumulation within Nuclei of Moss Leaf Cells

THE accumulation of lead within vegetation growing near busy roads has for some years received considerable attention¹⁻⁴. Less effort, however, has so far been made to find out where in the plants accumulation takes place, an important question when the subsequent effects of heavy metal pollution are to be considered.

We based our study upon the electron scattering properties of heavy metals such as lead. It was postulated that as lead is used as a stain in routine electron microscopy the presence within cells of lead derived from pollution might be revealed directly from transmission electron micrographs, provided that the local concentration of the metal in the cell was high enough and that no heavy metal stains were used during the initial preparation. As bryophytes are known to accumulate heavy metals in concentrations which are toxic for other groups of plants^{5,6}, mosses were chosen as the experimental material.

In a pilot study, specimens of *Rhytidiadelphus squarrosus* (Hedw.) Warnst. were cultivated in a greenhouse and watered once a day for three weeks with a series of lead acetate solutions (providing concentrations of 100–1,000–10,000 p.p.m. of lead). A control series was watered with tap water. Leaf samples were taken at one-day intervals. They were pre-fixed in 2.5% glutaraldehyde for 4 h, post-fixed in osmium tetroxide for 3 h, rinsed in cacodylate buffer after each fixation, dehydrated in acetone, embedded in 'Epon' and sectioned on an 'LKB-ultratome'. A 'JEM 100B' transmission electron microscope was used.

A comparison of electron micrographs from unstained polluted material with micrographs from leaf cells from the control samples revealed electron-dense inclusions in the cells from the lead-treated samples, while no such inclusions were found within cells from the control series. These electron-dense inclusions could also be recognized after post-staining the sections with uranyl acetate or lead citrate so as to enhance the contrast.

A similar investigation was made on lead-polluted material of *R. squarrosus* collected from a park in Trondheim about 250 m away from a relatively busy city road.

Within the nuclei of the leaf cells we repeatedly found electron-dense particles. Fig. 1 shows numerous inclusions

and damage to the nuclear membrane. Fig. 2 shows a leaf cell from this same moss cultivated under unpolluted conditions in the laboratory.

The electron-dense inclusions found within the nuclei of moss cells from the park area were believed to represent lead complexes, an assumption based on the following arguments: (1) Chemical analysis of the moss leaves showed a lead content of about 50 p.p.m. (dry weight). (2) Similar inclusions within cell nuclei are not known to be a normal part of plant cell ultrastructure. (3) When mosses from the same locality as those containing the electron-dense inclusions were cultivated in Petri dishes under normal "unpolluted" conditions in the laboratory, they developed shoots in which no similar electron-dense inclusions were found within the cell nuclei. (4) In an autoradiographical study, Dallenbach⁷ showed that isotopic lead (²¹⁰Pb) was incorporated into the cell nucleus. In a cell fractionation study, using tracer technique⁸, the lead isotope was found to be incorporated in the organelles of the nuclear and mitochondrial fractions. (5) In a series of fine structural investigations of experimental

Table 1 X-Ray Analysis of Electron-dense Inclusions

Peak (Pb L _α line) (P)	Background (b)	P-b
263	76	
312	57	
295	71	
290	60	
299	64	
Mean	292	66
		226

The height of the characteristic L_α line for lead is given as the difference between the peak value (P) and the background (b). This difference, P-b, represents a direct measure for the quantity of lead present in the analysed material.



Fig. 2 A leaf cell from the same moss species grown in unpolluted conditions. Lettering as in Fig. 1.

lead poisoning with rats⁹⁻¹², similar inclusions were found within cell nuclei of the tubular cells of the kidney. Goyer isolated these inclusions and found that they consisted of a lead-protein complex.

To validate our claim, an X-ray analysis of the electron-dense inclusions, using an 'EMMA' (electron microscope microanalyser), was carried out by the AEI consultant laboratory. The analysis confirmed that the electron-dense particles found within the nuclei did contain lead (Table 1).

Other areas in the cytoplasm which showed electron-dense precipitates were also examined, but with a single exception no lead could be demonstrated. In this one area the mean characteristic peak was 46 compared to the background of 22, giving a P - b value of 24. The aim of our study was to investigate the possibility of locating heavy metal residues—in this case lead—derived from atmospheric pollution within plant cells and organelles, using transmission electron microscopy. Our findings that lead is incorporated into the nuclei of lead-polluted moss cells agree with previous findings of lead inclusions within the nuclei of tubular cells from the kidneys of lead poisoned men and animals⁹⁻¹³. The binding of lead within the nuclear membrane, as a non-diffusible complex, has been suggested as the mechanism whereby the cytoplasmic concentration of diffusible lead substances within the cell can be kept below a level that would otherwise be toxic to the mitochondrial and other lead-sensitive functions of the cytoplasm, or as stated by Goyer: "This mechanism may be relevant to man's adaptation to his chronic body burden of lead". We suggest that the same mechanism may apply to plants.

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The Single Atom Spectrometer and its Application in Chemistry and Biology

MUCH work has been done on damage in solids caused by particulate and non-particulate radiation; however, the subsequent effect on the environment has been neglected. I have demonstrated¹⁻³ that material can be transferred to an emitter whose electrons bombard a solid, and that single atoms and complexes liberated can be detected by the fluctuations they cause in the field emission when they impinge on the emitter. The present work was directed to detecting, recording and identifying these entities; in short, to the design of a single atom spectrometer.

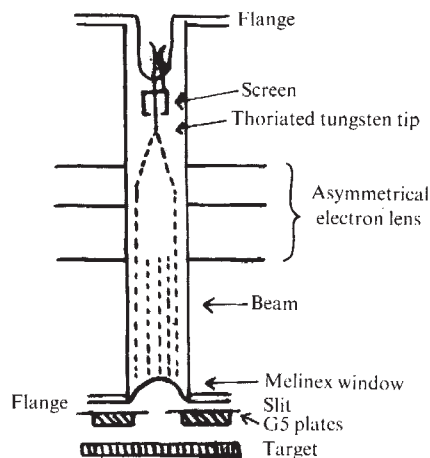


Fig. 1 Experimental tube.

In the experimental work, the radiation source was an etched thoriated tungsten point of some 3000 Å radius, from which field electrons are emitted into a good vacuum by an external asymmetrical three element electron lens used in the "Einzel" condition. The nearly parallel beam (current density $\sim 1 \mu\text{A cm}^{-2}$ at 55 kV) emerges through a 'Melinex' window and bombards the target in air. This is placed below a masked-off slit collimating system, $15 \times 1 \text{ mm}$, with Ilford G5 emulsion plates on its underside, 0.5 mm from the slit, as a detector. The scheme of experimental arrangement is shown in Fig. 1 where the spacing between the emulsion and target was kept at 0.1 mm and a field of $3\text{--}6 \text{ kV cm}^{-1}$ was placed between target and slit system.

When the plates are processed, they show the blackening

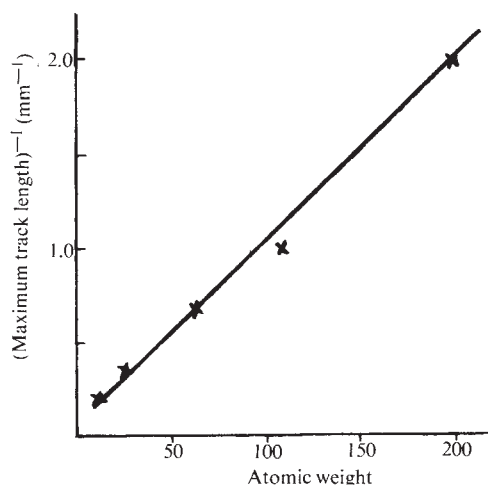


Fig. 2 Relation between $(\text{maximum track length})^{-1}$ and atomic weight.

expected near the edges closest to the slit from back-scattering effects, together with what appear to be a series of tracks of variable length, some curved, some straight, all of which seem to be characteristic of the target material and the impurities it contains. On the simple basis of momentum exchange, it would be expected that atoms or complexes liberated by bombardment would have a whole spread of energies almost up to the maximum of the incident beam. These would not travel any measurable distance in the emulsion, but, if channelled through the air, in directions nearly parallel to the surface of the emulsion, would be expected to have path lengths of a few tenths of a millimetre; indeed, the track lengths in the emulsion are of these magnitudes.

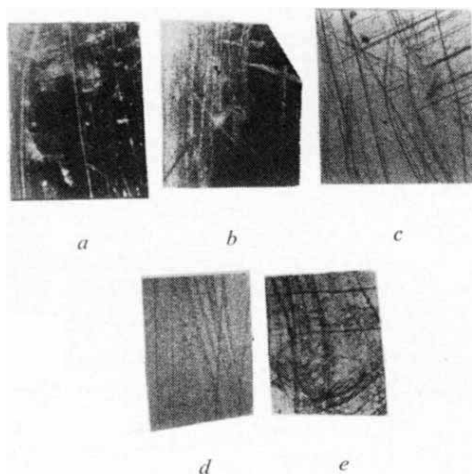


Fig. 3 a, Ammonium carbonate; b, anthracene; c, ox liver; d, human blood; e, γ -globulin. $\times 40$.

As, for a fixed energy, the energy loss of similarly charged particles varies with the mass, the range in air should vary inversely as the mass. It was found that, using targets of carbon, aluminium, copper, silver and platinum, the corresponding maximum track lengths in the emulsion also varied approximately inversely as the mass as shown by the rough calibration of Fig. 2.

A tentative proposal explaining the presence of tracks and accounting for the observed lengths is as follows. The back-scattered electrons from the target produce 'F' centres in the emulsion, and the available electrons are guided

through the emulsion by the field of those charged liberated atoms or complexes which happen to move in a direction very close to its surface (the field of a singly charged atom at some 10 Å from the surface is of order 10^9 V m^{-1}). The tracks in the emulsion would then delineate the paths of the charged liberated particles in air, and their lengths would give a measure of the masses of the atoms or complexes which produced them.

The interest for the chemist lies in the possibility of detecting impurities in his materials on an atomic scale, disintegration products of radiation damage or preferred orientations of released particles. For the biologist, the technique offers a method for following up changes in living cells under different biochemical conditions, radiation damage in tissue, the effects of missing atoms or groups on reactions of biological importance.

The compounds so far studied include graphite, ammonium carbonate, glucose, phenol, apiezon grease, paraffin wax, iodoform, iodine, urea, thiourea, salicylic acid, ox liver, egg white and yolk, β -naphthalamine, human blood, γ -globulin, and anthracene. As examples, tracks from the last three compounds together with those from ammonium carbonate and ox liver are shown in Fig. 3. The preferred direction of liberated atoms from anthracene is parallel to the slit edge and the majority of tracks show liberated carbon atoms; the longest are identified with liberated protons. The possibility of recording impurity atoms present in metal specimens is revealed by the study of tracks obtained with laboratory specimens of metals.

I thank ICI (Welwyn Garden City) for supplying the 'Melinex' plastic for the window. Full details will be published elsewhere.

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Dinoflagellates near the Cretaceous Jurassic Boundary

MANY well-preserved dinoflagellate fossils occur in Jurassic and Cretaceous marine sedimentary sections of various lithologic characteristics in the western North Atlantic and the western side of the Sacramento Valley of California^{1,2}. We have discovered five species which are common to the two sections—as first noticed by W. R. Evitt—each occurring in the Cretaceous above the Cretaceous/Jurassic boundary, and not in the subjacent Jurassic (Fig. 1). These species are *Biorbifera johnewingi* Habib, *Dingodinium cerviculum* Cookson and Eisenack, *Scriniodinium attadalense* Cookson and Eisenack, and two as yet unnamed species, *Polysphaeridium* sp. and *Pyxidiella* sp. Two of the five species, *B. johnewingi* and *Polysphaeridium* sp., were found to range through most of the Berriasian (lowermost stage of the Cretaceous System) at its type locality in southern France. *B. johnewingi* satisfies the requirements of a stratigraphically useful guide fossil especially well; it is morphologically distinctive, geographically widespread, relatively abundant in the three areas of investigation, and of limited stratigraphic range (Berriasian-middle Valanginian). Furthermore, it first appears very near the Cretaceous/Jurassic boundary at the three localities. A sixth species, *Occisucysta?* sp., appears to be discordant, being confined to the uppermost Jurassic in California and the lowermost Cretaceous in the North Atlantic.

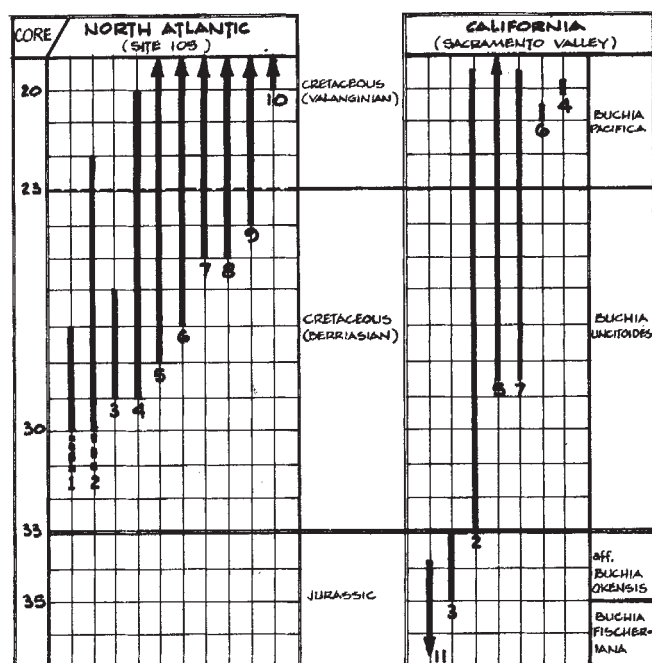


Fig. 1 Comparative dinoflagellate stratigraphy, North Atlantic and California. 1, *Cyclonephelium* sp.; 2, *Biorbifera johnewingi*; 3, *Oecisucysta*? sp.; 4, *Pyxidiella* sp.; 5, *Polysphaeridium* sp.; 6, *Scriniodinium attadense*; 7, *Dingodinium cerviculum*; 8, *Scriniodinium campanula*; 9, *Cyclonephelium distinctum*; 10, *Oligosphaeridium complex*; 11, *Belodinium dysculum*.

The fossils discussed above were observed at the four sites which yielded Lower Cretaceous sediments in the North Atlantic³. At site 105, which was drilled at abyssal depths approximately 380 miles east of Cape Hatteras, North Carolina (34° 53.72' N; 69° 10.40' W), coring was nearly continuous through partly recrystallized greyish-white to white nannoplankton chalk, and passed through the Cretaceous/Jurassic boundary. Based on calpionellids, the upper part of core 33 (Fig. 1) is considered to be very near the boundary, with preference to the lower Berriasian⁴. Dinoflagellate cysts are numerous and diverse through the Cretaceous down to core 30, below which they are rare. The interval between this core and the calpionellid boundary in core 33 is approximately 27 m. Dinoflagellates are abundant again lower down in samples of undisputed Jurassic age (core 35). None of the five species listed above was found in these samples, even though the assemblages are equally fossiliferous.

Table 1 Geographic Distribution of Stratigraphically Important Lower Cretaceous Dinoflagellates

Species	California	N. Atlantic	S. France
<i>Biorbifera johnewingi</i>	x	x	x
<i>Polysphaeridium</i> sp.	x	x	x
<i>Dingodinium cerviculum</i>	x	x	
<i>Pyxidiella</i> sp.	x	x	
<i>Scriniodinium attadense</i>	x	x	
<i>Scriniodinium campanula</i>		x	x
<i>Cyclonephelium distinctum</i>		x	x
<i>Cyclonephelium</i> ? sp.		x	x
<i>Oligosphaeridium complex</i>		x	x
<i>Diacanthum hollisteri</i>		x	x

Four other species that first appeared in the Cretaceous at site 105 are also present in the Berriasian type section, though not in California (Table 1). These are the following: *Cyclonephelium distinctum* Deflandre and Cookson, which ranges through most of the type section, *Cyclonephelium*? sp., which is apparently restricted to the lower and middle parts of the type section; and *Scriniodinium campanula* Gocht, which was found in the uppermost sample of the

type Berriasian and in the Valanginian above; and *Diacanthum hollisteri* Habib. In addition, *Oligosphaeridium complex* (White) is present in the Valanginian at the Berriasian stratotype locality, as it is also at site 105.

Dinoflagellates are common and well preserved in the Upper Jurassic and Lower Cretaceous rocks of the Great Valley sequence of California. This sequence is located on the west side of the Sacramento Valley, and consists of up to 40,000 feet of interbedded grey sandstones and dark grey mudstones, with numerous layers of limestone concretions⁵. The Upper Jurassic (Tithonian) through lower Lower Cretaceous (Berriasian-Valanginian) portion has been subdivided into six zones, based upon the succession of species of the pelecypod genus *Buchia*⁶. *Buchia* is generally a northern form, being found in California, the Pacific northwest states, Canada, the Russian platform and northern Europe, but not, however, in the stratotype sections of the Jurassic and Cretaceous stages in southern Europe south of northern France. Therefore, any age determinations based solely on *Buchia* may be suspect. Fortunately, ammonites are present in the Great Valley sequence, and afford fairly precise age determinations of the *Buchia* zones⁷.

In California, *B. johnewingi* appears virtually at the base of the Cretaceous System. *Polysphaeridium* sp. and *D. cerviculum* first appear at the top of the lower third of the Berriasian, somewhat earlier than their first appearance in the North Atlantic. *S. attadense* and *Pyxidiella* sp. have more restricted ranges, and appear higher in the section; however, they occur within their corresponding North Atlantic ranges. An additional species, *Belodinium dysculum* Cookson and Eisenack, is confined to the Tithonian, just below the boundary. This species was described as "probably Tithonian" from the Upper Jurassic of Australia⁸.

We conclude that the five Cretaceous species occurring mutually in California and the North Atlantic, as well as the additional species discussed in this report, are potentially valuable guide fossils. *B. johnewingi* is the most important, because it appears directly above the Cretaceous/Jurassic boundary in the two sections and also in the lower part of the Berriasian stratotype section. The similar succession of species in widely separated areas, and their close relationship to the Cretaceous/Jurassic boundary, emphasizes the contribution dinoflagellates make to Mesozoic stratigraphy on the continents and in the ocean basins.

We thank R. J. Davey and H. P. Luterbacher for samples of the Berriasian stratotype section. We also thank numerous colleagues for critical reviews of the manuscript. This work was supported by grants from the Deep Sea Drilling Project and the National Science Foundation.

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BOOK REVIEWS

English Science Policy

The Organisation of Science in England. By D. S. L. Cardwell. Pp. xii+268. (Heinemann Educational Books: London, October 1972.) £2.80. Revised edition.

FIFTEEN years after Dr Cardwell's *The Organisation of Science in England* first appeared we now have a revised edition (a paper-back version of which is issued as an Open University set book). Although it incorporates a good deal of recent scholarship on the social history of 19th-century British science, the argument of the book remains fundamentally unchanged.

The process by which English science was transformed from the "amateur" pastime of 1800 into the professional enterprise of the present day is the story of success over remarkable adversity. Reliance on individualism, voluntarism and self-help was reinforced by a deep-rooted suspicion of State intervention which made Victorian men of science initially reluctant to demand Government support of research. Government, on the other hand, did not, until quite late in the 19th century, recognize scientific research as something essential to national welfare. As late as 1869 the Chancellor of the Exchequer refused a request for a mere £300 from a meteorological society with the icy pronouncement that it was "our duty not to spend public money to do that which people can do for themselves".

Yet throughout the century statesmen of science like Babbage, Playfair, Tyndall and Huxley were vigorously arguing the dependence of national progress on the endowment of research, both pure and applied. What was good for the professionalization of science was good for Britain and State subvention was necessary. In 1878 Colonel Strange claimed that "the tendency of progressive civilization must be to supersede individual effort", and four years later Sir B. C. Brodie announced, with typical hyperbole, that a class of professional researchers "does not exist

among us and its absence is the greatest defect in our social system". Continental models came to the aid of a characteristically British capacity for self-criticism. German liberality in the support of science and applied science was the most frequently cited example of what Britain ought to be doing for her men of science. But it was not until the Great War that the consequences of British parsimony became fully evident.

The most intriguing theme of Dr Cardwell's book is the congruence between the professionalization of science and the reform of English education. While scientific training of the highest quality was always available to the upper classes, it was not until elementary, secondary and technical education was vastly expanded that England could produce the requisite "rank and file" of the scientific enterprise as well as the "officers". And, until the lower levels of education received adequate support, English science could not offer trained men of science the teaching positions which professional science requires.

Cardwell's work is still the most thorough and competent treatment of the subject available. Good as it is, it may be criticized on two main heads. First, it must be recognized that the relevant policy for professionalizing 19th-century science is Britain and not England. The bleak portrait Cardwell paints of the early institutional standing of science might have been quite different had the Scottish situation occupied more than a peripheral place in his account. Second, one feels that Cardwell has been unnecessarily harsh about early century "amateurism" and "dilettantism". The spread of scientific culture across the British landscape, notably in the form of the "literary and philosophical societies", very probably had quite a lot to do with emerging public approval of science as something to be encouraged and supported. "Amateurism", and a favourable public image of science, was probably a neces-

sary pre-condition for later "professionalism". Cardwell focuses on the politics of professionalizing science but gives the reader very little feeling for the social and cultural background against which the process unfolded.

STEVEN SHAPIN

Pattern Recognition

Fundamentals of Pattern Recognition. By Edward A. Patrick. Pp. xxiv+504. (Prentice-Hall: Englewood Cliffs, New Jersey; Prentice-Hall International: London, 1972.) £9.

THIS book is concerned with purely statistical approaches to the design of systems which, given specimen vectors belonging to distinguishable clusters or classes, will automatically classify any further given vector of the same dimensionality. The classification of the specimen vectors may or may not be known *a priori*, and in cases where it is unknown we are confronted with the essential problem of numerical taxonomy.

A 115-page chapter is devoted to techniques for estimating statistical parameters. The other main topics of the book are decision rules, non-parametric techniques, clustering techniques, and dimensionality reduction. The mathematical level is fairly advanced, and the treatment is for the most part thorough, rich in theoretical insight, and unusually coherent. Anyone who has fairly advanced mathematical accomplishments and is specifically interested in the theory of the statistical approach to pattern recognition will find this book valuable: in some respects more so than *An Introduction to Statistical Pattern Recognition* by K. Fukunaga (Academic Press, New York and London, 1972).

When patterns are subject to severe distortion, it is seldom useful to apply statistical techniques directly to digitized versions of the data. It is usually better to apply statistical techniques to vectors whose components are results of distortion-invariant tests on the raw data. To

select adequate tests which are more or less invariant to distortion, it has been necessary in practice to use *a priori* intuitive knowledge of distortion. This knowledge is of vital practical importance and has to be imported from outside a purely statistical framework such as Dr Patrick's. Dr Patrick is aware of this: he constantly advocates making maximal use of *a priori* structural knowledge, and in chapter seven he introduces a method for testing hypothesized structural relationships. But this does not tell us positively and specifically how to deal with highly correlation distortion. Because distortion raises by far the biggest fundamental and practical difficulty in many applications, and because Dr Patrick scarcely tackles this difficulty, a safer title for his book would have been 'Fundamentals of Statistical Pattern Recognition'. J. R. ULLMANN

Cerebral Iconography

An Illustrated History of Brain Function. By Edwin Clarke and Kenneth Dewhurst. Pp. 154. (Sandford Publications: Oxford, 1972.) £5.50.

MEDICAL-historical iconography is not only a pictorial history of observations, discoveries and cures but also, and perhaps more informatively so, of ideas. Nothing can be more explicit about this than the remarkably well devised and annotated atlas of historical illustrations of brain function under notice.

Looking into the history of ideas, the most interesting point is that it took only a little more than fifteen years to perform the long jump from rigidly unrealistic and schematic mediaeval delineation of sensory perception, imagination, rational thinking and memory in circles on the surface of the head to the realistic-observational and basically modern representation of the brain (Magnus Hundt, *Anthropologium*, 1501, to Laurent Fries, *Spiegel der Artzney*, 1518). Moreover this was achieved twenty-five years before Vesalius's *Fabrica* and a little more than twenty years before such pre-Vesalians as Dryander. The mediaeval idea was that of "cellular location", that is, the reservation of the brain ventricles for the functions: sensorium and sensory-conditioned imagination in the largest (lateral—"anterior") spaces, rational thinking in the narrowest sieve-like (middle) and memory in the most remote, posterior cell. This idea was brought about by a strange mixture of ancient Alexandrian and Galenic *pneuma* (spirit)—theories with materialistic connotations linking certain functional differences with different regions and textures. It appealed to the Fathers of the Church (Nemesius) and ever

since ruled for more than a thousand years.

As the authors most aptly show, it had not died with the opposition by the great anatomists, but survived in texts and illustrations not only with the mystical crypto-mechanist Fludd, but into the times of phrenology in the early nineteenth century when the mediaeval model was suitably revised and reproduced in the translation of Blumenbach's *Physiological Institutions* by John Elliotson (1789–1868). Talking about progress we are well reminded at the end of the mediaeval chapter in the caption of a normal ventriculogram of today that the "cells" are now known to be receptacles of the cerebrospinal fluid, but that "the exact nature of its production and function is still not fully understood". Nor would it be otiose to probe into the ideological motives that account for the popularity and long persistence of ventricular localization—a subject naturally outside the scope of the present work. A certain mediaeval—schematic—touch remains in at least one of the illustrations of the persistently overrated physiological speculator Descartes and even some of the ingenious and often under-rated observationalist Vieussens. On the ideological side Thomas Soemmering (1755–1830) in spite of fundamental contributions including pictorial masterpieces to brain anatomy "reverted to the mediaeval theory of ventricular function when he contended in 1796 that fluid in the brain ventricles could be animated and was the immediate organ of the soul". His *Über das Organ der Seele* was inspired (though also criticized) by and dedicated to Immanuel Kant ("unserm Kant gewidmet vom Verfasser").

And so we come by way of full and highly interesting chapters on Eastern models and phrenology in its own right to the scientific investigation of cerebral convolutions in the nineteenth century, via Flourens, J. Hughlings Jackson, Fritsch and Hitzig, and the cyto-architects to Penfield's direct cortical stimulation studies and a final extensive assessment of cortical localization today — ending with isotope radioactive brain scanning (illustrated).

Here, then, is a wealth of pictures synoptically integrated with a full historical, medical and artistic discussion. William of Conches's description of the brain (O'Neill, Y. V., *Clio Med.*, 3, 203; 1963), Tiedemann's illustrations of the cortex in negroes (influential in the controversies around the slave trade), Kurt Goldstein's attack on anatomical localization on a broad front against a background of deep insight, neurological as well as general-biological, and the changing experimental techniques from Joh. Gottfried Zinn to W. Feldberg deserve additional

attention. Nothing like that, however, can detract from the distinct merit of this most admirable treatment of the subject including the elegant atlas of impressively evocative illustrations.

WALTER PAGEL

Microphysical Phenomena

Quantum Physics and Ordinary Language. By T. Bergstein. Pp. xii + 61. (Macmillan: London and Basingstoke, October 1972.) £3.

QUANTUM theory treats of microphysical, and therefore not directly observable, phenomena. This raises the problem of language and its descriptive powers. We have only two concepts—wave and particle—describing space-time movement as established in classical physics. These concepts must serve for describing the phenomena on the atomic level and below it where, however, they are found to be complementary. The two concepts are mutually exclusive in the sense that they cannot be used in the same descriptive statement without incurring contradiction. They are exhaustive of the universe of discourse since together they suffice to describe all the phenomena that can be observed.

This book deals with the conceptualization of experience as first given by Niels Bohr by his principle of complementarity. Ultimately, all laboratory experiment, whatever its object and however "abstract" the theory on which it is based, must come down to the level of classical physics and ordinary language since the scientist can only read meters and clocks. The fundamental concepts of classical theory, like space, time, and cause, cannot be used without restriction in quantum mechanics. A physical observation implies an action which is an integral multiple of the quantum of action, h . The uncertainty principle precludes the use of classical concepts in the quantum domain while we cannot do without them in the laboratory. The situation presents us with an "aporia" as known from standard philosophy. Complementarity overcomes it but at a price: there is an interdependence of phenomenon and concept. The dividing line between phenomenon and instrument, or between object and subject, is movable since no meaning can be assigned to a result apart from the measuring method by which it is obtained.

Objectivity is expressed by the use of ordinary language and of classical physics. If this view is accepted, then there is always a part of experience that cannot be made "objective". The complementarity between observation and

definition of an object, or between the definition of a concept and its applicability, makes it impossible to prove the objectivity of ordinary language as a whole. A kind of Gödel inconsistency appears that forces us to accept natural language immediately, at least in part, in order to make science "objective". All the same, complementarity does not limit natural science but, in fact, extends its scope through the movability of the boundaries of the dual concepts.

Much of what is said in this book follows Bohr's original arguments. I wonder whether it is not necessary, after almost fifty years of discussion of the quantum-mechanical principles, to start on a new approach, for example, by means of the concept of information. The very extensive, and often illuminating, literature on the subject is also not considered by the author. This makes the monograph rather less interesting than it might have been.

E. H. HUTTEN

Basic Thermodynamics

Classical Thermodynamics. By D. Elwell and A. J. Pointon. Pp. 300. (Penguin: Harmondsworth, Middlesex, October 1972.) £4.

THIS is a good introduction to thermodynamics which deals with the three laws, basic kinetic theory (viscosity is not mentioned) and various applications to phase changes, magnetic and dielectric materials and thermoelectricity. The last part deals with thermal measurements. Students will obtain a good working knowledge of thermodynamics from this book.

There is little departure in the exposition from what may be called the usual treatments. Complications which arise in the proofs of the equivalence of various forms of the third law are not mentioned: in such arguments hypothetical materials should be considered for which the entropy-temperature curve approaches the $T=0$ axis at two separated points, thus ensuring unattainability of the absolute zero without providing a unique entropy value at $T=0$. Also, the one-page exposition of Carathéodory's principle is somewhat confused, and ought to have been omitted. The statement here that states which are adiabatically accessible from a given state "are limited to a surface in (P, T, v) -space" is false, and encourages confusion (which I believe is still too widespread) between an adiabatic change and an isentropic or quasistatic adiabatic change. But these are marginal points of criticism of an otherwise useful exposition.

P. T. LANDSBERG

Brain Nutrition

Lipids, Malnutrition and the Developing Brain. (A Ciba Foundation Symposium jointly with the Nestle Foundation in Memory of Sir Norman Wright.) Pp. viii + 326. (Elsevier/Excerpta Medica/North Holland: Amsterdam, London and New York, 1972.) Dfl. 40; £4.80; \$12.50.

As a general rule the published versions of the symposia of the Ciba Foundation are valuable additions to most libraries and this volume is no exception. It is well produced, illustrated and indexed, and the editors have almost captured the atmosphere that these small and usually productive symposia generate.

The topic of the symposium is one of vital interest not only from the scientific point of view but also because of the far-reaching implications that the experimental work with animals and the observations on malnourished children have for mankind in general.

While it is true that some of the material in the volume can be gleaned from the literature its assembly in one volume is useful. The discussions following each paper which are also included, as is the custom, give the main participants the opportunity of expanding their ideas in a slightly less formal way than in their papers.

The symposium covered a wide field but one major theme and two related themes were dominant. The first concerned the implications of the timing of malnutrition in relation to the chronological development of the brain.

The two other themes were related to the first but deal in greater detail with the biochemical changes associated with dietary treatments on the processes of myelination, and more especially with the effects of essential fatty acid intake on the formation of brain lipids, including those involved in myelin formation. All the contributions in these areas show how the complex chemistry of the brain lipids is yielding a rich harvest for the biochemist and biophysicist.

The papers given at this symposium show that knowledge of the basic biochemistry of brain tissue is advancing rapidly, but that the changes produced by malnutrition cannot yet be related to the functional properties of the brain.

The two papers concentrating on behaviour emphasize the difficulties involved in relating the biochemical effects of malnutrition with behaviour both from the conceptual viewpoint and also when one attempts to apply them to man where cultural effects further confuse the issue.

This in no way implies that the tone of the symposium is negative; it is extremely provoking in pointing to the

difficult areas which remain to be explored.

One is left with the feeling that the "extra-curricular" discussions at this symposium must have been exciting and stimulating for researchers in this field, and that for the non-participant this volume represents the next best thing to having been there.

D. A. T. SOUTHGATE

The Evolving Brain

The Brain in Hominid Evolution. By Phillip V. Tobias. Pp. xv + 170. (Columbia University: New York and London, February 1972.) £4.75.

THIS book, based on the author's 1969 James Arthur Lecture on the evolution of the human brain, is surely the most complete and up-to-date compilation of the volumetric data of the brain casts of fossil man and the extant pongids. Tobias, perhaps wisely, has not ventured into the difficult, if not dubious, study of cerebral morphology guessed from plaster endocasts, nor has he pursued any substantive questions of comparative neuroanatomy, beyond the most cursory examination of variables such as neurone number, size, density, dendritic branching, and glial/neural ratios. The main parameter discussed is that of brain size, as reflected by the volumetric capacity of endocasts.

The first six chapters are a compilation of data existing for living primates and the fossil hominids. Some of the data, particularly of the East African hominids from Olduvai Gorge, are previously unpublished. The last five chapters deal with the problem of interpreting the 1,000 ml. growth in brain size during hominid evolution, particularly in terms of primate problem solving abilities, and the prehistoric artefacts associated with the hominids. Throughout, this book is written clearly, even delightfully, and the ample tables, illustrations, and photographs are certainly first-rate, a tribute to Tobias's colleagues, particularly Alun Hughes, who helped in the preparation of this book. The only outright error I could find in this book was a reference to one of my own publications (1969a, "the role of the brain in human mosaic evolution", which through some strange chain of events was never actually published in the *Eighth International Congress of Anthropological and Ethnological Sciences*, Tokyo, 1968). Thus the reference should read "unpublished manuscript", but that is not Tobias's fault.

The evolutionary picture that Tobias draws, on the basis of the few endocranial remains of the hominids thus

far discovered, is one of a fairly singular and almost constant advance of cranial capacity and cultural complexity (based on stone tools), from an unknown size of *Ramapithecus* to *Australopithecus* to *Homo habilis* to *Homo erectus*, and so forth. This picture is surely oversimplified, for it excludes any dynamism or change of selection pressures beyond that associated with cultural complexity. Unfortunately, the major component of a cultural adaptation, namely the use of arbitrary symbols or symbolization, is never explained in any neural terms. Indeed, the whole issue of selection for body size and allometric growth of the brain is ignored, except in relation to Tobias's continuing use of Jerison's concepts of the "extra neurones" in the cortex, which are produced in tables for the hominids, including the East African *habilines*. This is not really a criticism, however, for the problem eludes all of us, and the book retains a clarity that would have otherwise been lost if this consideration had entered the discussion.

I am naturally gratified to find my own name and works so often referenced. I am curious, however, why Tobias continues to use a value of 500 ml. for the Taung specimen, when both he and Alun Hughes corroborated my results of 442 ml. for the adult, based on a value of 405 ml. for the child. All three of us found that my midline was accurately placed within one millimetre. I also must demur regarding any "chicken-egg" argument with Washburn over the role of the brain in human mosaic evolution, as I only pointed out that reorganization of the brain along human lines had taken place very early in human evolution, which is not the same as saying "the brain evolved first". Both Bielicki's (1969) and my (1967) papers, utilizing Maruyama's discussion of deviation-amplification and feedback between brain and cultural evolution, form the basis for the synthesis that Tobias attempts to bring forth near the end of the book; but why did Tobias not find my original synthesis between brain size, internal cortical structure, and the evolution of behaviour of some use, beyond a general citation?

The volume has much to present in exemplary fashion, but one cannot help feeling that the interpretative task still remains to be worked out, and that it is this aspect of the book which is perhaps a little weak. For example, Tobias offers the following schema for parameter interrelationships: increase in brain size $\Rightarrow$ increase in complexity of internal organization $\Rightarrow$ changing functional patterns $\Rightarrow$ changing patterns of behaviour. Unfortunately, this model is only partially discussed, and one is left with the impression that Tobias indeed sees the first and last items as

ends of a chain. Is this possible, or should it really be a continuous cycle of interrelatedness without ends? Certainly these two "ends" of the chain should be studied.

While this book by Tobias is an admirable and much needed compilation of recent data and perspectives on the evolution of the human brain, the serious student should closely examine many of the references given for himself, and should remain sceptical of several of the values given for the East and South African hominids, regardless of who did the determinations. This latter *caveat* is most important because the methods of ascertaining the volumes, particularly those based on partial endocasts, have not yet been fully tested for their accuracy. This is particularly applicable to the Olduvai Hominid 16, and to some of the South African fragments, such as MLD37/38 and STS 19/58.

R. L. HOLLOWAY

Algebraic Geometry

Introduction to Algebraic Geometry. By Serge Lang. Pp. xi+260. (Addison-Wesley: Reading, Massachusetts, 1972.)

THIS is an unchanged reprint of the book with the same title first published by Interscience in 1958. It gives an introduction to algebraic geometry, including the following topics: Places, algebraic sets, k -varieties, (affine, projective, abstract) varieties, Zariski topology, correspondences, product varieties, derived normal varieties, linear systems of divisors, differential forms, simple points, algebraic groups, Riemann-Roch theorem on curves.

This book does not cover the intersection theory, Grassman varieties, Chow varieties nor equivalence relations of cycles.

Chapter I contains basic theorems on valuation rings. Chapter II deals with algebraic sets, k -varieties and product varieties. Chapter III introduces the Zariski topology. Chapter IV considers geometric correspondences and the notion of abstract variety. Chapter V contains Zariski's main theorem for birational correspondences. Divisors and linear systems of divisors are dealt with in Chapter VI. Chapter VII deals with differential forms, Chapter VIII with simple points and Chapter IX with algebraic groups and abelian varieties. The Riemann-Roch theorem on algebraic curves is proved in Chapter X. Most chapters contain some useful references to literature. The book could well be used for a specialist course on algebraic geometry at the final honours or at postgraduate level.

T. J. WILLMORE

Keratins

Keratins: Their Composition, Structure and Biosynthesis. By R. D. B. Fraser, T. P. MacRae and G. E. Rodgers. Pp. xi+304. (Charles C. Thomas: Springfield, Illinois, June 1972.) \$16.75.

THE keratins are the α -keratins of mammalian epidermal appendages—hair, wool, horn and hoof and the feather keratin of birds and reptilian skin. They have played a central part as a material around which many of the key concepts in structural molecular biology have developed and are of more than academic significance to dermatologists, cosmeticians and, fortunately, the Australian wool industry. Part of the interest of this book is that it is essentially a product of the Division of Protein Chemistry of the CSIRO in Melbourne, a laboratory which has been largely responsible for modern views on the structure, composition and biosynthesis of keratins.

This volume reviews several aspects of the keratins. The histology and fine structure, the molecular structure and arrangement within the keratinized tissue, the physical properties and chemistry of the keratins are all described. There is an admirable didactic chapter on biosynthesis and a tantalizing concluding account of our woefully restricted understanding of the evolution of the keratins. The complexity of keratin structure is astonishing in view of their relatively inert biological function, and there are some suggestions in the book as to the reason for this.

This book fills the need for a comprehensive source-book on the keratins. The relevant data and arguments are presented and, in controversial situations a reasoned assessment provided. The production of figures and photographs is excellent.

ANDREW MILLER

Geography of Disease

Man, Environment and Disease in Britain: a Medical Geography through the Ages. By G. Melvyn Howe. Pp. xviii+285. (David and Charles: Newton Abbot, Devon, September 1972.) £4.75.

IN a way, this volume may be regarded as an extension of the author's previous book, the *National Atlas of Disease Mortality in Britain*. Here, in fact, Professor Howe sets out to look a little more closely at some of the variation charted in his atlas, and to attempt to compare the geographical variation with other differences of a biological, cultural and geological nature. He is also well aware that the dimension of time

cannot be ignored in such studies, and looks at population history as well as the comings and goings of various diseases in the past.

The text is liberally supplied with illustrations, usually in the form of schematic maps of the British Isles to show differences as regards such variables as temperature, rainfall, blood groups, mineral and water differences, pollution and industry and various diseases. These are certainly essential to a full appreciation of the points made in the text, and allow a brevity of comment which would otherwise not have been possible.

Mention should be made of one small error which could nevertheless give rather a wrong impression as regards the history of the treponematoses in Europe. In Plate 2, two views are shown of a skull from Spitalfields, in London, showing an advanced and extensive osteitis which could well be indicative of some form of treponematoses. But the date is not Saxon as indicated, and although there is still some debate as to whether it could even be pre-Saxon, it is far more likely to be Mediaeval in period. By a slip of the pen, it has also been attributed to the British Museum of Natural History, whereas in fact it is in the collection of the Department of Physical Anthropology in Cambridge University. Contrary, then, to what this specimen might seem to indicate, there is no certain case of treponematoses in Britain before late Mediaeval times.

All in all, this is a welcome addition to the geography of disease, and simultaneously is a contribution to human ecology and the history of medicine. Moreover, as a reference work, it will be useful to all those concerned with the reasons for biological variation in man.

DON BROTHWELL

Muscle Physiology

Electrogenesis and Contractility in Skeletal Muscle Cells. By J. Zachar. Pp. 638. (University Park: Baltimore, Md. and London; House of the Slovak Academy of Sciences: Bratislava, March 1972.) \$28.50.

DR ZACHAR makes for this book the modest claim that it is "intended for workers beginning in the field of general physiology of muscle contraction", and that for workers in other areas of physiology it "serves as a review of the present-day state of knowledge of electrogenesis and contractility of skeletal muscle". The first section of the book, dealing with the "molecular basis of contraction", achieves these aims in a satisfactory way, but this

material is already familiar from many books and reviews, and the treatment does not go beyond the level that the reader has been led to expect. Things change, however, when the author gets on to his own ground, namely the electrical behaviour of the membrane systems of muscle, and excitation-contraction coupling. Here each topic is dealt with thoroughly: theoretical treatment, historical background, and a summary and review of almost all of the recent papers are combined in a consecutive and readable text. All points of view are given a hearing, but Dr Zachar's judgment is always sound. Much quantitative material is collected from scattered sources into the thirty-seven tables which accompany the text. Key illustrations are reproduced from many papers. The book restricts itself to striated muscle; the emphasis is on single-fibre experiments, and as a result the great majority of the material is from the frog and from various crustaceans. Within this scope, very little is missed, as is suggested by the fact that the list of references runs to 1,200 items—complete with titles and final as well as initial page numbers. There are both author and subject indexes.

These features make the book a first-rate guide to the literature of membrane properties and excitation-contraction coupling in striated muscle. As such, it will be valuable not only to those who are starting out on research, but also to established workers in the muscle field. I doubt whether there are even any specialists who know the literature so well that they would not find this book valuable, especially because it contains numerous references to papers in Czech and other Eastern European journals that are not well known in the West, and because it contains a fair amount of unpublished work from the Zachars' laboratory. For the beginner, and for the worker on other aspects of muscle, it will be invaluable. It is perhaps more detailed than is necessary in a review of present-day knowledge for biologists outside the muscle field.

According to the preface, the typescript was submitted for publication in 1968, and it is unfortunate that the book did not appear (at least in Britain) until the summer of 1972 (although it is dated 1971). Additions were, however, made in proof, and it is greatly to Dr Zachar's credit that the book does not now give the impression of being out of date. Its value will indeed increase as the years go by and reviewers concentrate on later material; much of the great quantity of experimental work of the 1960s and late 1950s that is brought together here would be lost if it were not for the service that Dr Zachar has done in writing this book.

A. F. HUXLEY

Volcanoes

Volcanoes. By Gordon A. MacDonald. Pp. xii+510. (Prentice-Hall: Englewood Cliffs, New Jersey, 1972.)

IN my opinion this is unquestionably the best descriptive book on the market about volcanoes. The author, who has himself contributed much to the progress of volcanology, has now succeeded, as few can, in making a serious book so readable that even the non-specialist can profit from and enjoy it. The book opens with personal reminiscences about two volcanic eruptions, and this is followed by an introduction to volcanoes and to the properties of magmas, the raw materials, so to speak, of volcanoes. The next 130 pages contain a well-written descriptive survey of the different products of volcanic action, namely lava extrusions and fragmental rocks of various kinds. A broad survey then follows of the types of volcanoes and their eruptions, illustrated by many eyewitness accounts, and a discussion on the subsurface structure of volcanoes, their geographic distribution and tectonic setting, and their effects (both beneficial and otherwise) on man. The book finishes with a catalogue of the 500-odd active volcanoes in the world and an appendix giving a concise summary of the petrology of volcanic rocks. The work is well illustrated, with 150 photographs and 120 maps and line drawings, and the bibliography of some 500 references is well selected and is complete to about the end of 1969.

There are very few errors or misprints but there is one broad criticism which must be made, namely that, in the interests of making the book readable, the content of quantitative volcanology has been reduced to a low level and its place taken by descriptive matter. To give one example, the term "isopach" has been carefully avoided, and although two isopach maps are reproduced no hint is given that such maps constitute one of the most powerful tools for the quantitative study of volcanic products. In short, though the book succeeds in conveying the excitement and drama of volcanic activity, it fails to portray the basis of careful measurements on which the understanding of volcanism must ultimately depend, and it offers no advice on how such measurements can be made. A further mild criticism is that there is nothing new in this book: it is purely a skilfully written review of the literature. Of course such a review must be welcomed in view of the widely scattered nature of volcanological literature and, as a review of the subject, this book is excellent; but it is somewhat of a personal disappointment to find no new ideas expressed, no new ways of looking at volcanoes, with the author "sitting on the fence" about such controversial

subjects as the nature and origin of froth flows and tuff lavas.

Despite these criticisms, this book is clearly a "must" for all students of volcanoes and volcanism, and must now be regarded as the standard source book on this subject.

G. P. L. WALKER

Differential Equations

Theory of Partial Differential Equations. By Melvin H. Liebertstein. Pp. xiv+283. (Academic: New York and London, September 1972.) \$16.50.

THIS book is intended to provide an introductory account of the theory of partial differential equations for graduate students of engineering, physics, and possibly also applied mathematics. It has been written in four distinct parts. Part I, which comprises five chapters, gives an outline of the classical development of the subject. This ranges from an elementary account of the theory of characteristics and of the classification problem in E^2 to boundary value problems for the Laplace equation and the heat equation. It also contains a chapter sketching the notion of a properly posed problem.

Part II comprises four chapters, giving some results for both linear and nonlinear equations. These include the Riemann method, distortionless transmission line theory, and a helpful introduction to the Cauchy-Kovalevski theorem. Parts III and IV between them contain six short chapters. Potential theory and the Cauchy problem for the wave equation in terms of a retarded potential occupy Part III. Part IV offers a cursory discussion of some more recent ideas: it attempts, in forty-eight pages, to discuss the significance of *a priori* estimates, error bounds in numerical solutions and the existence of L^p weak solutions, while at the same time offering an outline of some functional analysis prerequisites.

Although an attempt to make the theory of partial differential equations better understood by engineers and physicists is to be welcomed, it is very doubtful that this book will be the vehicle by which this is achieved. It is certainly an interesting attempt, but it is too short, too selective, and too uneven in level. Thus on page 12 the reader is expected to know the meaning of the logical conjunction symbol $\wedge$, yet in the previous pages a first year derivation of D'Alembert's solution is offered. This unevenness of presentation mars an expository text, as does the failure to discuss first order systems, the absence of any systematic account of integral equations or of variational methods, and inconsistency of spelling (Kovalevski(y)).

A. JEFFREY

Liquid Metals

Liquid Metals: Chemistry and Physics. Edited by Sylvan Z. Beer. (Monographs and Textbooks in Materials Science.) Pp. x+731. (Marcel Dekker: New York, 1972.) \$19.50.

ON page one of this work there appears the remark "... physicists and chemists may not be familiar with the formalism commonly used by metallurgists ... the goal ... is to outline the formalism and to explain some of its characteristics". This is an excellent spirit in which to start a volume containing review articles by physicists, metallurgists, and chemists written for the benefit of all research workers in the liquid metals field. Let me say at once that the book will be of great value to any laboratory seriously interested in liquid metals, and it is no detraction to add that in any case there is nothing similar available.

The work deals with science, not technology. Fifteen articles, by experts in their fields, span most of the topics one would expect to find. The main emphasis is on comparing observations with theory, but there are substantial sections on experimental techniques and a useful amount of tabulated data. Perhaps the most conspicuous absentee is inelastic neutron scattering and thermal motion but, on the other hand, there are unexpected fringe benefits in the articles on solid non-crystalline alloys and evaporation kinetics.

As a physicist I was grateful for the lucid introduction to metallurgical thermodynamics in chapter 1, and metallurgists and chemists should be correspondingly pleased with the excellent articles on optical, magnetic and electronic properties. Most of the equations one is likely to want in diffraction analysis are set out by Wagner who deals extensively with multi-component systems and with technical questions like error analysis. Very readable chapters are available on viscosity, diffusion, and on electro migration—a subject "once considered mysterious" as the author puts it. The varied fortunes of significant structure theory applied to liquid metal properties are illustrated by Breitling and Eyring while Lee and Lichter attempt to relate thermodynamic properties with electron transport in alloys—an ambitious effort not wholly convincing to me. Sound propagation, high pressure effects and surface tension are the remaining subjects, all providing much of interest.

The printing and presentation are clear and the separate systems index is a useful feature. The book needs two complements: an expository textbook conceived as a whole and displaying the subject logically, and a collection of more recent material from

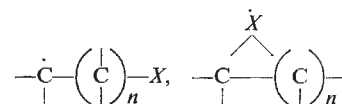
the past two years. The recent book by Faber, reviewed elsewhere, promises to supply the first, and before long the Proceedings of the Second International Conference on Liquid Metals will supply the second. Since the subject appears to be on a somewhat gently rising plateau of achievement at present, all these three different volumes promise to be useful for several years at least.

N. E. CUSACK

Radical Intermediates

Bridged Free Radicals. By Leonard Kaplan. Pp. xv+481. (Marcel Dekker: New York, September 1972.) \$24.50.

THE subject of bridging in free radicals [that is, the relative importance of the radicals



and radicals intermediate between them] has to be considered when rationalizing product distribution, kinetics, energetics, and spectra from many free radical reactions. Currently (see *J. Amer. Chem. Soc.*, **94**, 6550; 1972) the importance of bridged radical intermediates continues to be subject to much controversy.

A large quantity of information relevant to the subject of bridged radicals is buried in the literature, and this book intended as a comprehensive source for reference and teaching is to be welcomed. The various definitions of bridged radicals are critically discussed, and the book has chapters on silyl-, germyl-, stannyl-, chloro-, bromo-, iodo-, sulphur-, cyano-, and vinyl-substituted radicals. Selected data relevant to particular bridged radicals are briefly and sometimes critically discussed, and the inclusion of comprehensive tables, with extensive footnotes, makes it possible for the reader to draw his own conclusions, as is the intention of the author. This may be possible for readers expert in free radical chemistry, kinetics, stereochemistry, thermodynamics and spectroscopy; however, a person requiring to know if a particular radical is likely to be bridged may be overwhelmed by the welter of tabulated experimental results on which much effort must be expended to arrive at a meaningful conclusion. The mass of detailed tables, the continual necessity to refer to footnotes and to earlier sections, make the book difficult to read. In my opinion the book will be primarily of value to the expert in the field of bridged free radicals. The non-expert, be he teacher or student, may well be baffled by the scarcity of summaries and definitive conclusions.

D. I. DAVIES

CORRESPONDENCE

DDT and Pelicans

SIR,—*Nature* (240, 248; 1972) reports Dr Robert Risebrough's allegation that reproductive failure of the brown pelican in 1969-72 on Anacapa Island was due to DDT pollution "from a Los Angeles factory which in 1970 was discharging 500-1,000 lb of DDT daily into the sea".

This is a charge that Dr Risebrough has been making against Montrose, the factory he is referring to, for the last two years. I have sent letters to a California legislative committee and to Dr Risebrough himself outlining Montrose's treatment of its effluent since its start of manufacture more than 25 years ago. We have always settled DDT effluents in a settling pond, hauling the DDT sediments to a dump that accepts such wastes once or twice a year. The effluent to the county sewer line has contained entrained DDT equivalent to about 10-15 pounds per day. In May 1971, Montrose achieved essentially total recycle of its wastes and its discharge is about 1 oz of DDT per day.

The discovery of brown pelican reproductive failure was made shortly after the large oil-spill at Anacapa in 1969. W. Hazeltine (*Nature*, 239, 410; 1972) and Switzer and Wolfe (*Nature*, 240, 162; 1972) point out the invalidity of the attempted correlation of DDE residues and eggshell thinning.

At recent Washington DC DDT hearings, cross-examination of Dr Risebrough and others who hold to this correlation theory clearly demonstrated that without exception they used unreliable statistical approaches. Others, who have applied valid statistical analysis, such as Switzer for terns, demonstrated no such correlation.

Dr Risebrough's charges are becoming entrenched in the scientific literature. When such charges are referenced often enough, they become irrefutable "facts". I would hope that *Nature* will not allow itself to become part of this process.

Yours faithfully,

MAX SOBELMAN

Montrose Chemical Corporation,
Torrance, California 90507

Creation in California

SIR,—Although I hesitate to claim to be an expert "in a field of science bearing on the evolutionary theme", and therefore have not been able to take up your offer (*Nature*, 239, 420; 1972), I have recently been involved in teaching and research on chemical taxonomy, and

this has enabled me to look afresh at the subject of evolution. This has been done against a background of work on biogenesis which has certain similarities to evolution, for in it an attempt is made to determine how simple molecules have evolved into more complex ones during the life of the plant. Fifty years ago phytochemists speculated on possible pathways of biogenesis, some most fruitfully, but no one accepted these ideas as fact (or "truth") till innumerable experiments, checking and rechecking claims, have established some pathways with reasonable certainty. Looking therefore afresh at evolution, the most striking feature I find is the highly speculative nature of the topic; the situation phytochemists (in their obviously much more limited sphere of interest) were in 50 years ago.

For instance, one would have expected that classification of plants and animals would have been one of the first subjects to benefit from Darwin's ideas, as here at last was the basis for a "natural classification". Some modern plant taxonomists do not agree and feel that nothing has been fundamentally effected. "What is the impact of Darwin's ideas on taxonomy? It is commonly stated, or implied, that they were revolutionary. No taxonomist who has ever given a moment's thought to what he is doing in comparison to his predecessors can believe this. He knows that taxonomy in its broad outlines and in its more detailed practice has hardly been affected by evolutionary ideas. There is a curious dishonesty about this in much biological writing. Either the fact is denied, or it is glossed over, as if it is something to be ashamed of". In fact some seem to suggest that evolutionary ideas have been a hindrance to taxonomy. "In the absence of complete fossil records, so-called 'phylogony' is deduced from taxonomy. Attempts to base taxonomy on 'phylogenetic criteria' generally involve circular arguments, and it is now belatedly coming to be realized that evolutionary speculation has had a deleterious effect on practical taxonomy". I find it difficult therefore to accept your opinion that "Darwinism occupies a place in science at least as strong as that of Newton's Laws".

Clearly, the situation is no fault of the biologists; if adequate facts are not available they have to do with what they have. But it amazes me why biologists seem wedded to the rather limiting assumption that God must have created life only once. Could He not have

repeated the operation, on various scales of complexity, several times, thus explaining the great gaps between phyla and such insoluble problems as the origin of the angiosperms? I should guess that one of the motives behind the "Californian Creationists" is to ensure that young people are aware that the Bible, *inter alia*, at least suggests this possibility. Like many of the people referred to by Dr Hayward³ I treat the Genesis account of creation with as much respect as that of the biologist. Perhaps we are not too far removed from Darwin, who ended his *Origin of Species* with these words: "There is a grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one whilst this planet has gone cycling on according to the fixed law of gravity. . . ."

Yours faithfully,

J. W. FAIRBAIRN

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29/39 Brunswick Square,
London WC1N 1AX

¹ Walton, S. M., in *Chemical Plant Taxonomy* (edit. by Swain, T.), 6 (Academic Press, London and New York, 1963).

² Watson, L., Williams, W. T., and Lance, G. N., *J. Linn. Soc.*, 59, 492 (1966).

³ Hayward, A. T. J., *Nature*, 240, 577 (1972).

Doctrine of Creation

SIR,—The argument as between the theory of evolution in general and Darwin's theory of natural selection in particular on the one hand, and the account of the creation in the book of Genesis on the other, is surely of no religious interest. But that such an argument should exist is of profound significance for science.

All scientists know that it is not the purpose of science to arrive at ultimate truth. Science is nothing more than a systematic description of phenomena by means of "theories", commonly expressed in the form of "laws". None of these theories and laws is sacrosanct; all are provisional.

As a small boy, early in the century, I was brought up in an old-fashioned school. We were taught that the biblical story of the creation and the subsequent events in the garden of Eden was an allegory. The allegorical truth of that story still remains unshaken. That is so in spite of the fact that I sometimes feel I can detect certain ideological overtones in the writing.

That scientists should still be arguing

about these things is saddening. It betrays a pathetic lack of scientific faith¹.

Yours faithfully,

V. B. WIGGLESWORTH
Gonville and Caius College,
Cambridge

¹ Wigglesworth, V. B., *Ann. Appl. Biol.*, **60**, 1 (1967).

Noah's Haemoglobin

SIR, — In the ecumenical interest (*Nature*, **239**, 420; 1972), we wish to point out that the latter portion of Genesis is consistent with current theory of molecular evolution.

Kimura (*Nature*, **217**, 624; 1968) and King and Jukes (*Science*, **164**, 788; 1969) propose that protein evolution has occurred mainly through neutral mutations which have no effect on molecular function and no selective advantage. Haigh and Maynard Smith (*Genet. Res.*, **19**, 73; 1972) have examined this theory with respect to the published data on human haemoglobin variation. They conclude that if the theory is correct the human population must have passed through a period of drastically reduced size prior to the more recent rapid population increase. The

population bottleneck is necessary to give initial genetic homogeneity, a requirement for agreement of theory and observation.

Genesis, chapter 6, contains a full description of events causing a reduction in the human population to eight individuals; Noah, his wife, their three sons, Shem, Ham and Japheth, and the sons' wives. It seems entirely plausible that this small population could be homogeneous for haemoglobin genes. Thus the book of Genesis documents a series of human population changes which are consistent with changes required from consideration of amino-acid sequences alone.

Yours faithfully,

RICHARD N. HARKINS
PETER STENZEL
JOHN A. BLACK
Department of Biochemistry and
Division of Medical Genetics,
University of Oregon Medical School,
Portland, Oregon 97201

Titles and Reprints

SIR,—As a librarian, I can easily answer the puzzle postulated by Briggs and Briggs¹ as to why more requests were received for a paper published in *Contraception*² than for the one in

*Nature*³. *Nature* is more readily available. Virtually all scientific libraries have at least one subscription to *Nature*; whereas relatively few subscribe to the highly specialized *Contraception*.

Even though photocopying is cheaper than mailing a request, without the journal at hand a reprint is necessary.

Also as a librarian, I want to admonish the editor of *Nature* for publishing a paper with a deliberately misleading title. This paper will turn up as a false drop for years to come. The use of titles has become a powerful literature searching technique⁴. It is bad enough that uninformative titles are still being written through ignorance, but to write phoney titles intentionally is just outright perverse.

Yours faithfully,

DONALD A. WINDSOR
Biological Research Department,
SciAesthetics Institute,
Post Office Box 604,
Norwich, New York 13815

¹ Briggs, M. H., and Briggs, M., *Nature*, **240**, 490 (1972).

² Briggs, M. H., Briggs, M., and Austin, J., *Nature*, **232**, 480 (1971).

³ Briggs, M. H., and Briggs, M., *Contraception*, **3**, 381 (1971).

⁴ Windsor, D. A., *J. Chem. Document.*, **11**, 227 (1971).

Announcements

University News

Dr William Robert Dearman, Newcastle upon Tyne, has been appointed to a professorship in engineering geology from August 1, 1972.

Dr Alan Williams, Leeds University, has been appointed Livesey professor and head of the Department of Fuel and Combustion Science from January 1, 1973.

Dr G. C. Morrison, Illinois, USA, has been appointed to the Chair of Nuclear Structure at the University of Birmingham.

Professor Roger Penrose, Birkbeck College, London, has been appointed to the Rouse Ball Professorship of Mathematics, at the University of Oxford, in succession to Professor C. A. Coulson, Oxford.

Appointments

Dr Edwin Clarke, MD, FRCP, has been appointed Director of the Wellcome Institute of the History of Medicine as from October 1, 1973.

Miscellaneous

The **Lomonsov Gold Medals for 1972**—the highest award of the Soviet Academy of Sciences, awarded annually for out-

standing achievements in the natural sciences to one Soviet and one foreign scientist—have been awarded to **Academician Nikolsi Ivanovich Muskhelishvili** for his work in mathematics and mechanics, and to **Professor Max Steenbeck** (German Democratic Republic) for his research on plasma physics.

Professor Arpad I. Csapo, Washington University, has been awarded the 1972 award of the **International Health Foundation** on "Prospects for Practical Application of Prostaglandins".

International Meetings

The University of Texas Health Science Center, Houston, announces the Tenth Symposium on Biomathematics and Computer Science in the Life Sciences to be presented at Houston, Texas, March 1973. (Dr Stuart Zimmerman, University of Texas.)

Reports and Publications

not included in the Monthly Books Supplement
Great Britain and Ireland

Agricultural Research Institute of Northern Ireland. Forty-fifth Annual Report, 1971/1972. Pp. 43. (Hillsborough, Co. Down: Agricultural Research Institute of Northern Ireland, 1972.) [2910]
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